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ABSTRACT

The review of social and behavioral science programs supported by the National Science Foundation recommends improvements in program substance and management. Sixteen social and behavioral scientists who are or have been grantees under Foundation programs served on the review committee. They examined more than 150 documents pertaining to behavioral and social sciences, received advice, information, and suggestions from over 300 individuals and groups, and evaluated information from the NSF on research funding, grant management, scientists, research projects, and Foundation policies. The booklet outlines the scope of NSF programs, evaluates basic research by discipline, recommends new opportunities and interdisciplinary directions, discusses and evaluates the program on Research Applied to National Needs, compares NSF with other social science programs, and assesses the impact of research policies on universities. Sixteen findings and recommendations are presented, including: (1) quality of basic research projects is generally excellent; (2) many important investigations are unfunded or underfunded; (3) expensive and long-term projects tend to be discouraged, rejected, or reduced; and (4) program planning should have more input from the scientific community. Appendices provide data on publication of research supported by NSF, support for young researchers, and expenditures for facilities and equipment. (Author/DB)

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Social and Behavioral Science Programs in the National Science Foundation

FINAL REPORT

Committee on the Social Sciences
in the National Science Foundation
Assembly of Behavioral and Social Sciences
National Research Council

SP 010 312

NATIONAL ACADEMY OF SCIENCES
Washington, D.C. 1976

NOTICE

The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the Councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the Committee responsible for the report were chosen for their special competences and with regard for appropriate balance.

This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

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*Because of previous commitments, Drs. Neel and Westheimer were unable to serve through the entire period of the preparation of this report. The former accepted appointment to the Committee as of January 1, 1976, hence did not participate in the early stages of its deliberations. The latter could serve only until December 31, 1975, hence did not review the final version of the report.

CONTENTS

PREFACE 1
PRINCIPAL FINDINGS AND RECOMMENDATIONS 5
Support of Basic Research
Support of Applied Research
SCOPE OF THE PROGRAMS 9
Development and Scope of NSF Support 12
Basic Research
Applied Research
Relation of Basic and Applied Research 18
BASIC RESEARCH 23
Nature and Quality of Programs 26
General Evaluation 26
Quality of Research Supported
Opportunities Lost
Conclusion
Emphases of Current Program 32
Psychobiology
Neurobiology
Social Psychology
Anthropology
Linguistics
Economics
Human Geography and Regional Science
Law and Social Sciences
Political Science
Sociology
Special Projects
Social Indicators
History and Philosophy of Science
Science Policy
New Opportunities and Directions 44
The Data Base: Economic Behavior
The Data Base: Public Use Samples
The Data Base: Other Examples
Comparative Research
Interdisciplinary Workshops
National or Regional Facilities

Management of Basic Research 55
Proposal Review Panels
Program Planning
NSF Staff as Program Representatives 58
APPLIED RESEARCH 62
Social and Behavioral Science in RANN 63
RANN Programs in the Social and Behavioral Sciences 64
Energy Systems
Managing the National Environment
Disasters and Natural Hazards
Public Sector Technology
Public Policy and Economic Productivity
Public Policy and Human Resources
Technology Assessment
Intergovernmental Science and Research Utilization
Human Performance: A Neglected Research Area
General Evaluation and Conclusion 71
Routes to the Application of Knowledge 72
Orientation of the Directorate 74
Management of the Directorate 76
Stability of Programs
A Management Alternative
Use of the Applied Science Community
Staffing
A New Division
OTHER SOCIAL SCIENCE PROGRAMS 83
Office of National R&D Assessment
Science Education
Science Resources
IMPACT OF RESEARCH POLICIES ON UNIVERSITIES 85

APPENDIX A: PUBLICATION OF BASIC RESEARCH SUPPORTED BY THE NATIONAL SCIENCE FOUNDATION 87
APPENDIX B: SUPPORT FOR YOUNG RESEARCHERS IN THE BEHAVIORAL AND SOCIAL SCIENCES 92
APPENDIX C: FACILITIES AND EQUIPMENT SUPPORT IN THE BEHAVIORAL AND SOCIAL SCIENCES 98

PREFACE

The Committee on the Social Sciences in the National Science Foundation was established by the National Academy of Sciences, at the request of the National Science Foundation (NSF), to examine the scope and quality of the Foundation's programs in the behavioral and social sciences with a view to recommending possible improvements in both the substance of the programs and the procedures for managing them. This report of the results of the Committee's study is addressed to the Director of the Foundation and his associates, but we hope that it will also have value for others inside and outside the federal government who are concerned with NSF policy and programs.

Before presenting its findings and recommendations, the Committee wishes to describe its own background and point of view, so that its initial assumptions and biases are clear. This is not a citizens' committee, but a group of professional behavioral and social scientists (together with a chemist and a geneticist). The fact that we have devoted our careers to scientific research, both basic and applied, and to teaching means that there is nothing neutral about our attitudes toward science. We believe that scientific knowledge provides an essential foundation for modern society and that the preservation and further progress of that society calls for the vigorous pursuit of science, both basic and applied. The social sciences are an integral part of the total scientific endeavor, and they must and can be pursued with objectivity, respect for evidence, and intellectual sharpness.

It is not the purpose of this report, then, to judge the case for the behavioral and social sciences. The Committee was not appointed to undertake that task, nor is it properly constituted to do so. Rather, its aim has been to review NSF's programs in those sciences with a

view to suggesting how they can be improved. Some of the recommendations can be implemented with present resources, others would require additional funds. Whether such funds should be allocated can only be decided, of course, by Congress and the administration, when they have weighed the claims of behavioral and social science research against other claims for public support. We have tried to indicate what could be accomplished if certain programs were implemented.

Most of the members of the Committee have, at one time or another, received research support from the National Science Foundation. A majority of us are currently grantees under the Foundation's programs. Our involvement with NSF is itself an indication of the important role the Foundation plays in the support of basic and applied behavioral and social science research. It would be virtually impossible, even if it were desirable, to assemble a representative group of behavioral and social scientists who did not have such associations, past and present, with the Foundation.

All of us on the Committee have devoted much thought, over many years, to our own choices of research topics. Hence, we did not arrive at the deliberations of the Committee without views, in our respective areas of expertise, about what lines of inquiry were most worth pursuing. Another committee with different members might have different views, just as strongly held, about priorities. We have tried to guard ourselves against provincialism by consulting broadly and informally with many colleagues.

In general, the areas of consensus are much broader than the areas in dispute. As is pointed out in this report, ascertaining the consensus at any time is an important task for NSF itself, and performing that task well is crucial to doing a good job of reviewing research project proposals.

In summary, the members of the Committee are scientists who believe deeply in basic and applied science and its social importance. All of us began this inquiry with views about our own special areas, based on our professional experience in basic and applied research. We have tried to learn from the staff of the National Science Foundation, from each other, and from colleagues outside the Committee as we went along, but our final product must be taken for what it is: the considered collective judgments of a particular group of scientists, drawn from most of the social science disciplines and a number of areas within each of them, but not presuming to represent all our tens of thousands of colleagues.

The Committee on the Social Sciences in the National Science Foundation was formed in August 1975. During its first six months, Committee members examined more than 150 published and unpublished reports and documents pertaining to the behavioral and social sciences. The Committee reviewed and evaluated information available from the National Science Foundation on research funding, grant management, scientists, planned, ongoing, and completed research projects, and Foundation policies. The Committee also obtained comments on Foundation programs from research colleagues and other persons outside the Foundation. Committee members met formally in September, November, December, and January; in February 1976, a draft of this report was distributed for comment. The Committee met again in early May and considered those comments in preparing this final report.

The Committee is grateful for the assistance it received from many sources. Over 300 individuals and groups contributed directly to this report by offering expertise, information, and suggestions. We hope they will forgive our not mentioning each of them by name. Staff from all parts of the National Science Foundation provided data and talked with the Committee; we are grateful to them for the time and effort they expended. We owe special thanks to those persons in the Foundation who helped organize our initial information collection effort: Richard C. Atkinson, Harvey A. Averch, James H. Blackman, Eloise E. Clark, James D. Cowhig, Howard H. Hines, Richard T. Louttit, Patricia J. McWethy, and Thomas F. Sparrow.

The executive officers and secretaries of several professional associations were also of much help to the Committee. These individuals, Charles A. Kiesler, Evron M. Kirkpatrick, Edward Lehman, Fred C. Leone, Hans Mauksch, Warren Nystrom, Millard H. Rund, and Mark Thompson, offered suggestions and spread word of the Committee's activity to thousands of scientists. In response to our request for comments, many behavioral and social scientists, as well as other interested persons from education, industry, and government, wrote or spoke to Committee members and staff, providing us with many thoughtful comments and ideas. We are indebted especially to scientific colleagues who traveled to meet with Committee members; among them were Brian J. L. Berry and Julian Wolpert. Staff members of several Congressional committees provided the Committee information or access to information through the Congressional Reference Service; they include Thomas Kramer, Anne Strauss, and Robert Strauss.

Donald E. Stokes and Thomas K. Glennan, Jr., and their associates in the National Academy of Science's study project on social research and development were most generous in sharing with us their considerable knowledge of that subject and of the research programs in NSF. Members of the Executive Committee of the Assembly of Behavioral and Social Sciences of the National Research Council and the Report Review Committee of the National Academy of Sciences provided lengthy, thoughtful comments on drafts of the report. Finally, we thank David A. Goslin, Lester P. Silverman, Eugenia Grohman, and Christine Lintz McShane of the Assembly of Behavioral and Social Sciences, who helped to organize the Committee and prepare its report for publication; and Cherry Stewman at Carnegie-Mellon University, who guided several drafts of the report through the computer that produced them.

Our whole undertaking would have been infeasible without the solid background of information in the report of the Behavioral and Social Sciences Survey Committee (*The Behavioral and Social Sciences: Outlook and Needs*, Washington D.C., National Academy of Sciences, 1969). The BASS report, although now seven years old, remains a mine of fact and wisdom about the behavioral and social sciences.

Herbert A. Simon, Chairman
Committee on the Social Sciences
in the National Science Foundation

July 1976

PRINCIPAL FINDINGS AND RECOMMENDATIONS

The body of this report examines the National Science Foundation's programs of support for basic and applied research and their administration. This section provides a map for that examination by setting forth very briefly the most important findings and recommendations.

Support of Basic Research

1. The quality of basic research projects in the social and behavioral sciences supported by the Foundation is generally excellent, although some reservations about specific emphases are detailed later in this report. There is also a large number of excellent proposals that are rejected because they cannot be supported within the present program budgets.

2. Projects that require large-scale support (several hundred thousand dollars per year or more) or that need to be supported over a long period to produce results tend to be discouraged, rejected, or reduced.

3. Many important investigations of high quality are now underfunded or completely unfunded. Since only about 30 percent of the qualified investigators in the social and behavioral sciences receive federal research funds, the pool of available research talent is adequate for substantial expansion of activity. (An average of 58 percent of investigators in the other sciences receive federal funds.)

4. Evaluating the relative adequacy of NSF funding of basic research among the several social and behavioral

science disciplines is not easy, especially if support from other federal agencies is to be taken into account. Data on the proportion of proposals funded, the amount of research support per university scientist, and recent trends in total federal and Foundation support do not present an entirely consistent picture. On balance, however, researchers in psychology, political science, and anthropology may be faced with especially severe funding problems. The proposed fiscal 1977 budget, if adopted, would go some distance toward alleviating the stringency of funds in psychology, but would not significantly improve the situation in political science and anthropology.

5. No important areas of basic social or behavioral science research were identified that could not seek support under one or another of the existing programs. We commend strongly the proposed creation of the new programs for sensory physiology and perception and for memory and cognitive processes, separating these topics from the previously very heterogeneous psychobiology program. In the report's discussion of the individual programs, a number of specific questions of direction and balance are raised that require attention from program directors and advisory panels, but that probably do not call for formal reorganization of the program structure.

6. The report identifies a number of important research opportunities that have been insufficiently explored or exploited by the Foundation, generally because of the large scale of support or the long-term commitment they imply. These kinds of opportunities will continue to be neglected unless the staff of the Directorate for Biological, Behavioral, and Social Sciences encourages large-scale proposals when they are justified by the circumstances, reviews them carefully even when they cannot be funded within current budgets, and takes an active and vigorous role in seeking additional allocations of Foundation funds to support those that are meritorious. Some examples of such research opportunities are: longitudinal studies over extended time periods, comparative research across nations and cultures, and national research facilities for, for example, survey research, information processing psychology, psychoacoustics, large data bases, and advanced study.

7. Research involving laboratory experimentation or using advanced mathematical and other quantitative

techniques appears to receive relatively high priority in most of the social sciences. While this is an appropriate emphasis in the Foundation setting, greater support needs to be provided (particularly in social psychology, economics, sociology, and political science) for studies using techniques of field research and ethnographic analysis of social institutions and processes. Special encouragement should be offered to substantive studies that show good promise of advancing these latter methodologies and making them more reliable and informative.

8. Not all basic research programs now employ outside panels for the review of proposals. Panels should be created for all programs now lacking them and should be used regularly by all programs. Average panel ratings of proposals should be determined, and panels should be informed whether projects are funded. The program directors should provide explanations whenever they do not follow the recommendations of the review panels.

9. There should be more input from the scientific community in program planning. Ad hoc advisory groups should be used by program directors in planning future programs and budgets. Each advisory group should meet with the appropriate program director at least once a year.

10. It does not appear that the staff managing basic research in the social and behavioral sciences has effectively represented these sciences at the higher administrative levels of the Foundation and in the Foundation's negotiations with the administration and Congress. It is imperative that they perform these functions better.

Support of Applied Research

11. The Committee attaches great importance to the support of applied research aimed at solving significant problems in our society. The RANN (Research Applied to National Needs) program is a useful component of the federal government's support of applied behavioral and social science research, complementing and filling gaps in the programs of the mission agencies. However, the present RANN program needs strengthening. The RANN social and behavioral science research is highly variable in quality and, on the average, not impressive. Moreover, the scale

of the program is quite modest in relation to the range of appropriate research targets.

12. The RANN procedures for developing programs and proposal solicitations and for reviewing proposals do not provide adequately for the participation of applied scientists. Present procedures may be reasonably suitable for assessing the relevance of proposals to social goals, but more participation of the performer communities is essential for judging the scientific quality of proposed projects. Proposals should be funded only if they meet high standards of both relevance and scientific quality.

13. Achievement of the RANN applied research objectives is impeded by some aspects of the present RANN operation. In particular, RANN procedures now reflect excessive confidence in the ability of a few staff members to determine the proper direction for research programs. Staff pressure to tailor research proposals to narrowly specified programs, planned from the top down, plays too large a role in RANN funding decisions, and response to genuinely unsolicited proposals plays too small a role.

14. The roster of the behavioral and social science programs within RANN should be modified to correspond more closely to the structure of the applied fields that will carry out the research. Many of the present research activities could be better accommodated in programs for such interdisciplinary fields as public finance, organizational administration, operations research and management science, communications, public choice, urban affairs, human performance, land use and resource management, government regulation of industry, public law, program evaluation and measurement, or combinations of these fields.

15. Improving the quality of the research projects funded by RANN and their outputs requires greater stability of programs and longer-range financing of projects.

16. The Directorate for Research Applications needs greater participation by staff with training in the social and behavioral sciences. The need is particularly acute in those programs that fund technical research with important economic and social implications and in the higher administrative levels of the Directorate.

SCOPE OF THE PROGRAMS

Science is one of the powerful tools that humans have developed for deepening their understanding of the physical, biological, and social world around them; for increasing their ability to act intelligently and effectively in that world; and for enabling them to estimate the environmental and social consequences of their actions.

Since 1950 the major public responsibility for progress in basic science in the United States, and part of the responsibility for progress in applied science, has been vested by law in the National Science Foundation. At first limited primarily to the physical and biological sciences, this responsibility now also extends to the behavioral and social sciences, which today account for about seven percent of the total NSF budget.

The behavioral and social sciences are sometimes viewed as quite new and untried; in fact they have a long history. This year marks the 200th anniversary of the publication of Adam Smith's *An Inquiry into the Nature and Causes of the Wealth of Nations*, a great piece of economic reasoning. We are within three years of the 100th anniversary of Ebbinghaus's seminal studies of human memory. Nor was mathematics slow in being applied to human affairs. The first mortality tables were published by John Graunt in 1662, and the first calculations of life annuities based on such tables were carried out by the astronomer Halley in 1693, just seven years after he assisted Newton with the publication of the *Principia*. Cournot's pioneering work in mathematical economics appeared in 1838, a generation before Maxwell wrote out the basic equations governing electricity and magnetism. The National Science Foundation's support of research in the behavioral and social sciences is not, therefore, a novelty. Rather, it affirms a belief of three hundred years'

TABLE 1 Social and Behavioral Science in the Federal Government, Fiscal 1975 Estimated Obligations ($ in millions)

Federal Agencies	Social and Behavioral Sciences		Total Research	Total R&D
	Basic Research	Applied Research		
National Science Foundation	$ 31.3	$ 18.1	$ 591.4	$ 620.5
Department of Health, Education, and Welfare: Total	39.6	145.5	1,927.8	2,403.7
Alcohol, Drug Abuse, and Mental Health Administration	20.4	35.6	124.1	124.1
National Institutes of Health	10.1	15.2	1,587.9	1,868.2
Office of Human Development	–	18.2	24.2	73.6
Social Security Administration	1.9*	25.5	27.4	27.4
Office of the Secretary	4.4	22.9	27.3	27.3
Other	2.8*	28.1	136.9	283.1
Department of Agriculture	15.5	36.6	407.3	423.4
Department of Defense	15.5	36.1	1,766.8	8,859.5
Department of Justice	14.1*	22.0	47.2	64.1
Department of Housing and Urban Development	–	28.7	32.9	63.8
Department of Labor	1.1	17.4	18.5	23.0
National Aeronautics and Space Administration	2.2	9.1	1,564.4	3,065.3
Department of State	–	9.7	26.0	32.3
Smithsonian Institution	7.6	–	24.9	24.9
Veterans Administration	0.3	5.6	89.6	98.4
Department of Commerce	0.08	4.8	150.7	210.1

Department of Interior	1.6	2.3	223.9	305.1
Department of Transportation	–	1.6	62.9	370.4
Civil Service Commission	0.6*	0.8	1.4	4.1
Advisory Commission on Intergovernmental Relations	–	1.2	1.2	1.2
Federal Trade Commission	–	1.2	1.2	1.2
Environmental Protection Agency	–	1.1	132.9	286.6
Federal Communications Commission	–	1.0	1.9	1.9
Other federal agencies	0.02	2.7	664.0	2,045.6
TOTAL	129.5	345.5	7,736.9	18,905.1

SOURCE: *Federal Funds for Research, Development, and Other Scientific Activities, Fiscal Years 1974, 1975, and 1976.* Vol. 24. Detailed Statistical Tables, Appendices C & D. National Science Foundation 75-323. Tables C-5, C-15, C-27, C-34, C-53.

NOTE: Funding estimates and classifications in this table were derived from NSF figures. Data are provided in January of each year by 34 agencies and their subunits. Estimates are reconciled with budget documents and are also subject to further appropriations, apportionments, or allocation decisions.

The category of behavioral and social science includes the disciplines of psychology, anthropology, economics, history, linguistics, political science, sociology, law, and socioeconomic geography. Agencies classify basic and applied research according to its major purpose: gaining more fundamental knowledge, or practical use. Intramural research in federal agencies is included in the research category, as is research performed in universities, independent institutes, and other extramural facilities. Development is the use of research information applied toward the production of materials, programs, or methods. Experimental programs and evaluations are apparently included in the R&D figures, but some agencies classify them as research (e.g., Social Security Administration), whereas others classify them as development (e.g., National Institute of Education). Excluded from R&D are demonstration projects, collection of statistics (except for research purposes), scientific and educational information dissemination, educational information (e.g., preparation of classroom guides), funds primarily for the training of scientific manpower, and R&D facilities or permanent equipment.

*Asterisks denote research whose classification as "basic" is questionable on grounds of research purpose, review procedures employed, and problems studied. Subtracting these funds leaves $110.00 million for social science basic research. Also see text for the limitations of these data.

standing that the methods of thinking we call science are as applicable to human affairs as they are to physical phenomena and biological organisms.

Development and Scope of NSF Support

Basic and applied research in the behavioral and social sciences and development work relating to social problems are sponsored by a number of federal agencies. It is hard to arrive at an exact figure for NSF participation in this total support, since precise boundaries cannot be drawn around these activities--especially development work as distinguished from operating programs. One estimate is that NSF activities account for about 24 percent of the total federal support for basic research in the behavioral and social sciences, and about five percent of total federal support for applied research in those sciences (see Table 1).

Basic Research

The National Science Foundation is the second largest source of federal funding specifically designed to advance basic knowledge in the behavioral and social sciences; only HEW provides more support for this type of research (see Table 1). Support for the social sciences as a separate entity within NSF began in 1956 with the creation of the Social Science Research Program. In 1960, the program assumed the status of a division, which included separate programs in anthropology, economics, sociology, and the history and philosophy of science. The Division of Social Sciences later added separate programs in social psychology, geography, law and social sciences, political science, social indicators, science policy, and special projects, and linguistics has just been elevated to program status. The biological and individual aspects of experimental psychology were funded through programs in psychobiology and neurobiology in the former Division of Biological and Medical Sciences. By 1974, NSF had become the major source of support for basic research in many of these disciplines (see Table 2). Under the reorganizations of 1975, those programs are administered through the Directorate for Biological, Behavioral, and Social Sciences, and specifically through the Division of Behavioral and Neural Sciences and the Division of Social Sciences in that Directorate.

TABLE 2 Federal Obligations for Basic Research in the Social and Behavioral Sciences by Discipline and Agency

Discipline	Agency	Total Basic Research Fiscal 1975 Estimate		Basic Research in Colleges and Universities Fiscal 1974 Actual	
Psychology	Agriculture	$ 9,000		$	
	Commerce	70,000			
	Defense	13,087,000		7,083,000	
	HEW	24,831,000		17,210,000	
	Civil Service	643,000			
	NASA	2,189,000		466,000	
	VA	254,000			
	NSF	**6,063,000**	**(13%)**	**5,280,000**	**(18%)**
Economics	Agriculture	14,687,000		7,031,000	
	Commerce	13,000			
	HEW	3,052,000		4,364,000	
	Interior	203,000			
	Labor	1,122,000			
	NSF	**6,270,000**	**(25%)**	**4,945,000**	**(30%)**
Anthropology	Agriculture	4,000			
	HEW	357,000		426,000	
	Interior	1,088,000			
	Smithsonian	2,464,000			
	NSF	**4,100,000**	**(51%)**	**3,571,000**	**(89%)**
Sociology	Agriculture	658,000		390,000	
	Defense	462,000		161,000	
	HEW	5,052,000		5,856,000	
	Interior	79,000			
	Justice	10,600,000			
	Action	20,000			
	VA	9,000			
	NSF	**1,769,000**	**(9%)**	**1,750,000**	**(21%)**
Political Science	Agriculture	14,000			
	Defense	862,000			
	HEW			687,000	
	Interior	182,000			
	NSF	**1,407,000**	**(57%)**	**1,205,000**	**(64%)**
Linguistics	Defense	388,000			
	HEW	447,000		485,000	
	NSF	**1,450,000**	**(63%)**	**1,200,000**	**(71%)**
History	Agriculture	92,000		1,000	
	HEW	55,000		54,000	
	Smithsonian	5,144,000			
	NSF	**928,000**	**(15%)**	**850,000**	**(94%)**
Social Science	Agriculture	61,000		1,000	
Not Elsewhere	Defense	716,000			
Classified (e.g.,	HEW	5,769,000		3,178,000	
law, geography)	Interior	63,000			
	Justice	3,549,000			
	NSF	**9,263,000**	**(48%)**	**5,664,000**	**(64%)**

SOURCE: *Federal Funds for Research, Development, and Other Scientific Activities, Fiscal Years 1974, 1975, and 1976.* Vol. 24. Detailed Statistical Tables, Appendices C & D. National Science Foundation 75-323. Tables C-37, C-46, C-82, C-85.
NOTE: See text for the limitations of these data.

Table 2 shows that the pattern of agency support for basic behavioral and social science research varies considerably from discipline to discipline. The data in this table must be treated with great caution because each agency follows its own definition of basic research, and a great deal of research that is classified as basic would not be so classified by NSF or by us; nonetheless, the picture provided by the table is of some interest. The National Science Foundation accounts for a majority of the total federal support in anthropology, linguistics, and political science. Two-thirds or more of the support for economics, however, comes from other agencies as does 80 percent or more of the support for psychology and sociology. These differences are significant in two respects. First, the relative adequacy of support for basic research in different disciplines cannot be judged from the NSF budget allocations alone, but must be examined in terms of total federal support. Second, changes in the funding levels in other federal agencies have unequal effects upon the different disciplines, and hence varying impact on the NSF program.

Within NSF itself, about nine percent of the total budget for basic research is allocated to the behavioral and social sciences (see Table 3); however, nearly 24 percent of the nation's scientists who are engaged in basic research are associated with these disciplines. By this measure, basic researchers in the behavioral and social sciences receive, on average, a little more than one-third as much support as those in other sciences.

When we look at total federal support of basic research by all agencies, the picture is changed only slightly. The 24 percent of researchers who are behavioral and social scientists receive only 14 percent of research awards and less than nine percent of total federal research funds--again a ratio of nearly three to one. Including non-federal research funds does not materially alter the picture.

The situation is the same if one looks at federal funds for all research, applied as well as basic. Table 4 shows what fraction of research investigators in various fields receive some support from the federal government and what fraction of all research proposals submitted to NSF in these same fields are funded. The table provides further evidence of relatively low allocations to the behavioral and social sciences. Among the ten fields listed, the social sciences and psychology rank ninth and tenth, respectively, in the percentage of research proposals that

TABLE 3 Distributions of Basic Research Funds and Basic Researchers, Fiscal 1973

Fields of Science	Distribution of NSF Basic Research Funds[a]	Distribution of Basic Researchers at Universities and Colleges[b]	Distribution of Basic Researchers at Universities and Colleges Supported by Federal Funds[c]	Distribution of Current R&D Expenditures from Federal and Non-Federal Sources in Universities and Colleges[d]
Physical Sciences	29.4%	22.4%	24.7%	11.4%
Mathematics	5.6	10.9	6.9	2.5
Environmental Sciences	22.0	4.6	5.2	7.1
Engineering	10.3	5.1	5.8	13.0
Life Sciences	17.4	33.5	43.7	52.0
Other Sciences	6.5			3.6
Total Behavioral and Social Science	8.8	23.6	13.7	10.4
Psychology	2.5	7.8	6.6	2.5
Economics	1.2	2.9	1.1	1.6
Sociology/ Anthropology	1.6	5.0	2.6	2.1 (Sociology only)
Other Social Science	3.5	7.9	3.4	4.2
All Fields TOTAL	100.0 $392,400,000	100.0 67,800	100.0 34,800	100.0 $2,936,700,000

NOTE: Percents may not equal 100.0 due to rounding.

[a]Approximately 95 percent of NSF basic research funds are awarded to performers at universities and colleges. Data from *Federal Funds for Research, Development and Other Scientific Activities, Fiscal Years 1973, 1974, and 1975.* Vol. 23, Detailed Statistical Tables, Appendices C and D. National Science Foundation, NSF 74-320-A.

[b]Estimates based upon self-reported support and basic research as primary or secondary activity by scientists in colleges and universities (*Reviews of Data on Science Resources.* National Science Foundation, June 1975, NSF 75-310).

[c]Derived from *Reviews of Data on Science Resources,* National Science Foundation, June 1975.

[d]*Expenditures for Scientific and Engineering Activities at Universities and Colleges, Fiscal Year 1974.* Detailed statistical tables, Appendices B and C. NSF 75-318. (Above data are for 1973.)

TABLE 4 Federal Research Support in Ten Disciplines and Fields

	Employed Doctoral Scientists Supported by Federal Funds, 1973[a]	Rank	NSF Success Ratio, 1974[b]	Rank
Atmospheric Sciences	88%	1	70	2
Oceanography	82	2	75	1
Physics/Astronomy	65	3	56	3
Life Sciences	60	4	44	7
Earth	53	5	53	4
Engineering	51	6	41	8
Psychology	40	7	30	10
Mathematics	33	8	52	5
Social Sciences	30	9	38	9
Chemistry	27	10	49	6
Total Number	226,750		20,734	

[a]Data derived from the biennial survey of doctoral scientists and engineers in the United States conducted by the National Academy of Sciences. Compare *Characteristics of Doctoral Scientists and Engineers in the United States*, National Science Foundation, May 1975, NSF 75-312.
[b]The success ratio is the number of grants awarded expressed as a percentage of the number requested. Derived from *Success Ratios and Other Management Indicators*. Administration Directorate, National Science Foundation, November 1975.

are funded. In these fields, one proposal in three is approved, while nearly three-quarters in the atmospheric sciences are approved, and nearly one-half in the other physical sciences and the life sciences. Of course it is difficult to compare the average quality of proposals submitted in different fields or the extent of self-selection that operates in the submission process. However, the Committee examined a sample of the proposals that were found acceptable but could not be funded and concluded that the low percentage of support for social and behavioral science proposals must be attributed to the limitation of funds and not to a deficiency in the numbers of good proposals submitted.

Applied Research

In addition to its support of basic research, NSF supports considerable applied research in the behavioral and social sciences. The largest part of this is administered by the Directorate for Research Applications; another part, formerly administered by the Office of National R&D Assessment, is now administered by the new Division of Policy Research and Analysis in the Directorate of Scientific, Technological, and International Affairs. The core of the activity in research applications is the RANN (Research Applied to National Needs) Program. Activities related to social science in RANN began on a significant scale in 1971 with the creation of the programs on social systems and human resources (SSHR), and exploratory research and problem assessment. Applied behavioral and social science work has since been supported to some extent in all RANN divisions, most notably in the current Division of Advanced Productivity Research and Technology (into which SSHR was incorporated). Other administrative units in NSF support smaller amounts of behavioral and social science research, and the Directorate for Science Education supports activities designed to advance education in all fields of science.

Applied research activities are defined primarily in terms of the problems that give rise to them; these problems sometimes cut across scientific disciplines, calling for cooperation among scientists drawn from many different disciplines. For this reason, it is difficult to estimate what portion of the funds for applied research support work in the behavioral and social sciences. Our best estimate is that NSF in fiscal 1975 allocated $18.1 million

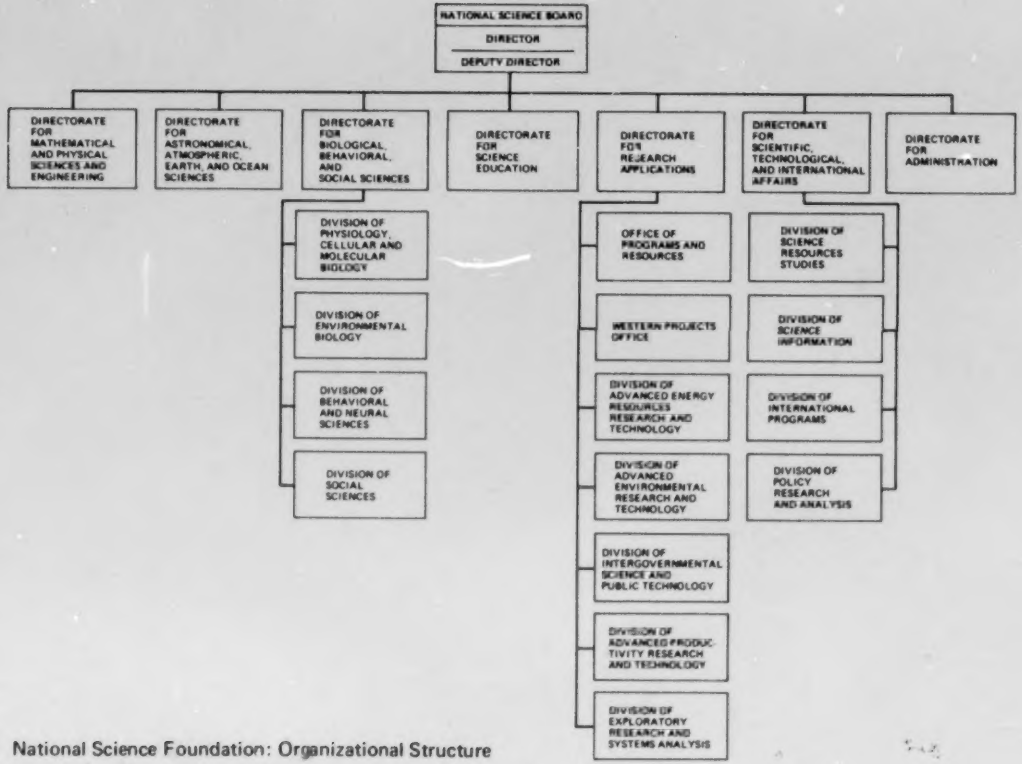
to such applied research and that all federal agencies allocated about $345.5 million, so that NSF provided about five percent of the total federal support for such work. The $18.1 million constituted about 20 percent of NSF's budget for applied research.

Relation of Basic and Applied Research

The present organizational structure of NSF makes a separation between basic research and applied research (see Figure 1). As noted, basic research in the behavioral and social sciences is administered through the Directorate for Biological, Behavioral, and Social Sciences, while applied research is administered mainly through the Directorate for Research Applications. The procedures employed by the two directorates in selecting research projects for support differ in a number of important respects.

Reflecting this organizational division, we devote separate sections of this report to each of these two areas. Although it would be difficult to discuss the present program in any other way, we have some grave reservations about making such a great distinction between what is basic and what is applied.

The usual criteria for distinguishing basic from applied research can be summarized in this way: The sciences, it is said, have their own internal logics, which largely determine what problems are next on the research agenda--what questions must be answered to make the next important steps forward, and what questions, in the light of present knowledge and technique, are likely to yield answers. The best basic research is research aimed at problems that are high on the internal agenda of science, either because of the fundamental nature of the questions addressed or because of the vital importance of the tools to be forged. The best applied research, on the other hand, is aimed at solving or helping to solve large and pressing human problems. However, applied research, like basic research, must be appraised not only in terms of the value its product would have if the inquiry were successful, but also in terms of the likelihood that it will be successful. In the present state of scientific knowledge, a proposal, for example, to develop recordings that students could use to learn foreign languages during sleep would not be thought a promising undertaking, however valuable the product if it succeeded: on the basis of what we already know about human memory, it is highly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DIVISION OF SCIENCE RESOURCES STUDIES
DIVISION OF SCIENCE INFORMATION
DIVISION OF INTERNATIONAL PROGRAMS
DIVISION OF POLICY RESEARCH AND ANALYSIS
DIRECTORATE FOR ADMINISTRATION

FIGURE 1 National Science Foundation: Organizational Structure

unlikely that learning could be induced during sleep in this way.

The criteria that determine priorities in basic and in applied research are not identical. A project might be ranked very high by the one set of criteria, but low by the other. It is easy, however, to exaggerate the magnitude of the difference, especially when we take a long-term or even middle-term view. As we have learned many times in the past several centuries, any basic discovery that advances and restructures knowledge in fundamental ways almost always has a correspondingly major impact upon applications of knowledge. Conversely, projects aimed at solving large and difficult practical problems can seldom succeed without pushing forward the boundaries of basic knowledge.

"Basic" does not mean "inapplicable" or "useless." Most scientists engaged in basic research are very much interested in potential applications of the new knowledge they are creating, but they are aware that these applications are not likely to come immediately. Society supports basic research because of a belief that fundamental advances in knowledge will lead to important practical applications, and conversely, that advances in practical knowledge and technology rest on the foundation of basic knowledge. The history of the past two or three hundred years provides a mass of evidence to support that belief.

The first successful open-heart surgery by John Gibbon in 1954, for example, required the development of much diverse knowledge beyond what was known 108 years earlier when ether anesthesia was first used to perform surgery. That knowledge included a basic understanding of the exchange of oxygen and carbon dioxide between gas and blood, which was needed to build an artificial apparatus that could take over temporarily the function of the patient's heart and lungs; an understanding of X-rays that was essential to the later invention of catheterization; and a modern theory of electrocardiography that was fundamental to the use of catheterization.

The boundary between basic and applied research is probably even less sharp in the behavioral and social sciences than it is in the physical and biological sciences. A great deal of the best work both advances basic knowledge and helps solve important practical problems. The advances in the basic theory of resource allocation, for which Professor Tjalling Koopmans of Yale recently shared the Nobel Prize in economics, were of this kind. Koopmans' initial discovery was made while he was doing

applied research on ocean tanker routing during World War II. It was expanded into both an important general reformulation of the basic economic theory of allocation, and practical planning techniques for industry that are now routinely used by business concerns as part of operations research or management science. (Koopmans' research was supported for many years by the Foundation. In the immediate postwar period, prior to the establishment of the NSF program in the social sciences, it received important support from the U.S. Air Force and the Office of Naval Research.)

This is not an isolated example. Adam Smith set the central task for economists when he titled his work *An Inquiry into the Nature and Causes of the Wealth of Nations*. The discipline of sociology was much stimulated by concern with human institutions like the family and with the problems of poverty and crime. Psychology received a major impetus in the United States from its engagement in research on personnel training and selection during World War I. Early anthropological research by the Bureau of Ethnology of the Smithsonian Institution dealt with the causes of such social conflict as the massacre at Wounded Knee in 1890. Some of the roots of linguistics research are found in the efforts of missionaries to bring the Bible to all people in their own tongues. Woodrow Wilson, while a young university professor, was one of the founders of public administration as a basic subject of inquiry within political science. The involvement of the behavioral and social sciences with research applications is no less visible today: wherever one looks, one sees close ties between basic inquiry and major economic, social, political, and psychological questions that face us as a society and as individuals.

In the social and behavioral sciences, application most often means using scientific knowledge to aid in making public policy decisions, for example, using information gained through economic research as an aid to reaching decisions about tax laws. Research applied in this way does not tell us what decision to make or whether to introduce a new program: it produces new or better knowledge about the alternatives. Application may also mean making changes in social institutions or introducing new practices into them, for example, new methods of reading instruction for school children or computerized procedures for scheduling complex construction projects in business and government. These new techniques or programs are based upon knowledge from social science research.

The too-casual application of new ideas emerging from social science research has the same potential for producing unanticipated and often unwanted consequences as does the premature application of natural science discoveries. New practices and institutional changes need to be accompanied by pretesting and analysis of their direct, indirect, and possible effects.

Furthermore, since many of the unexpected consequences of all science invention lie in the economic and social domains, the social and behavioral sciences have a special responsibility for evaluative research of all kinds. Because of this, a very large part of applied social science research is evaluation, providing knowledge about present and future effects that is relevant to the daily decisions confronting legislators and administrators.

We are still very far from being able to assess accurately the prospective consequences of technological and social change. Perhaps the most important contribution the social and behavioral sciences can make to society is to provide, through basic and applied research, the means for better understanding of the consequences and uncertainties of all kinds of change as well as the consequences of no change at all.

BASIC RESEARCH

As with the other basic research programs in the National Science Foundation, the programs in social and behavioral science operate largely through the review and funding of unsolicited applications. Some 2,000 research proposals are received each year, of which about one-third are funded; the rest are rejected or withdrawn. The vast majority of applications come from individual scientists or small groups of scientists in universities and in research institutes affiliated with universities; a few come from other nonprofit organizations.

For purposes of management and budgetary control, the total funds available for basic research in the social and behavioral sciences are allocated to 16 different programs. Last year, the individual program allocations ranged from about $0.5 million to $7 million (see Table 5). Each program is managed by a program director, who may have an assistant. The boundaries of some of the programs coincide roughly with the boundaries of established scientific disciplines. This is true of the economics, political science, sociology, and anthropology programs. The discipline of geography encompasses most, but not all, of the geography program--which has recently been renamed and slightly changed to the human geography and regional science program. The social psychology program lies largely within the discipline of psychology. The psychobiology and neurobiology programs are interdisciplinary: the psychobiology program supports not only research in psychology but also animal behavioral studies by zoologists and other biologists and studies in the genetics of behavior; the neurobiology program supports research in all areas of the neurosciences, a small part of which is carried out by behavioral scientists. The history and philosophy of science program spans two disciplines that

TABLE 5 Research Awards in NSF Behavioral and Social Science Programs, Fiscal 1975

Program	Research Awards	Doctoral Dissertation Awards	Total Awards	Average Research Award	Median Research Award	Number Research Awards Above $100,000
Economics	130	2	$7,257,700	$ 55,809	$51,666	10
Political Science	33	7	1,546,700	46,352	45,833	0
Sociology	45	10	2,776,800	61,168	54,166	8
Anthropology	100	70	3,996,900	36,566	31,765	3
Geography[a]	10	3	491,400	47,410	33,333	1
Social Psychology	44	8	2,201,700	49,377	50,909	0
Psychobiology	139	0	5,103,250	36,714	33,750	5
Neurobiology	135	0	5,386,400	39,899	42,115	1
History and Philosophy of Science	45	5	929,450	20,509	12,368	0
Science Policy	13	2	448,400	34,238	27,500	0
Law and Social Sciences	22	1	907,100	40,941	42,000	0
Social Indicators	14	0	1,667,950	119,139	90,000	4
Special Projects[b]	47	13	3,482,300	73,410	40,833	7

[a]Now, Human Geography and Regional Science.
[b]Including linguistics, which is now a separate program.

are quite separate at some universities, but unified at others. The science policy program lies mostly in sociology. The program in law and social sciences is multidisciplinary. Finally, the special projects and social indicators programs have large sociological components, but also serve to manage projects not elsewhere classified.

A recent reorganization separated linguistics from special projects, elevating it to program status; it overlaps substantially with anthropology, psychology, and computer science (artificial intelligence). A new memory and cognition program lies largely within the discipline of psychology, and a new program in sensory physiology and perception is interdisciplinary.

The management of a program is distinctly simpler when it lies within a single discipline than when it crosses disciplinary boundaries. First, in assessing the relative merits of different projects, there is less of a problem of comparing apples with oranges. Even if there is specialization within the discipline (as there usually is), most scientists have at least some acquaintance with all or most of the specialities and hence some competence to judge the significance of projects in them. When a program cuts across disciplinary lines, scientists on different sides of the boundary may evaluate priorities quite differently; indeed, the boundary may have arisen, historically, out of just such differences in viewpoint.

On the other hand, important new scientific directions often originate at the boundaries between disciplines--as linguistics did, for example, at the boundaries between anthropology, classical languages, modern language, and psychology, all of them established disciplines with quite distinct traditions. Organization along strict disciplinary lines may delay the recognition of the value of work at these boundaries.

The initial responsibility for recommending approval or rejection of NSF projects rests with the program directors. In discharging this function, they draw heavily upon advice from the scientific community (within the discipline or outside it), using two main channels to solicit that advice. In all cases, they obtain reviews of individual project proposals by mail. In general, there will be three or four such reviews, but sometimes as many as a dozen. Second, some of the programs have a review panel that meets periodically to review and make recommendations on all proposals received during the intermeeting period. Membership on review panels is rotated, each member serving two or three years. The role of both

mail reviewers and panel members is purely advisory; final responsibility for action remains with the program director. There are no formal procedures for reviewing the results of research once it is funded, except when an application for renewal of support is submitted.

The proposal review process has recently been described by NSF in a June 1975 staff study, "Peer Review and Proposal Evaluation." Also, the National Academy of Sciences, under contract to NSF, and the National Science Board are presently engaged in an extensive evaluation of NSF's review process. We will comment on that process here only as it affects the quality and direction of the behavioral and social science research programs.

Nature and Quality of Basic Research Programs

The Committee has devoted a great deal of its effort to reviewing the quality of the research funded by NSF, both basic and applied. It was not our intention to second-guess the many scientific colleagues who had passed judgment on the individual projects as members of panels or mail reviewers. Each of the thousands of proposals had been subjected to a far more thorough evaluation than we could give it. Although we did scrutinize the projects that were approved and a sample of those that were rejected, our aim was not to reevaluate individual projects, but to try to obtain a broader view of the agency's performance. What was the general quality of the scientific investigators who were funded and those who were not? What were the main thrusts of the programs? What impact did the research funded by the Foundation have upon the state of scientific knowledge in the disciplines? What were the effective priorities among areas within each discipline, and how well do those priorities correspond with the discipline's judgment of scientific promise and ultimate importance for application? Are there important areas within disciplines or in the gaps between disciplines that were not adequately supported? Are there particular kinds of research that have not been supported or have been supported inadequately?

General Evaluation

There are two distinct parts to this evaluation: the quality of the activities that have been supported and of

those that might have been--the values of the actual program and the costs of opportunities lost, risks avoided, and adventures abandoned. The next two sections of this report are devoted to these two topics.

Quality of Research Supported

We have used a variety of means to assess the quality of the research funded by NSF. Our evaluation rests in large part on our direct acquaintance with a significant fraction of the projects that are supported in our disciplines and fields. Wherever possible, however, we have tried to corroborate personal knowledge with pieces of objective evidence that could be collected. The picture from all sources is quite consistent.

The record of publication is one suggestive indicator of the quality of the research supported. Just as research proposals are reviewed and screened for funding, so research reports are refereed before being accepted for publication in scientific journals. Acceptance rates of articles submitted to journals are not unlike the acceptance rates of proposals submitted for funding; most leading journals commonly accept less than one-third of the articles they receive. The journal referees, like proposal reviewers, measure the research against the standard criteria--significance of findings and soundness of methodology.

Appendix A undertakes to provide some indication of how research supported by NSF fared in the publication process. As is explained in Appendix A, both the data and their interpretation are subject to some uncertainties. Nevertheless, we may fairly conclude from them that NSF-supported basic research in social and behavioral science fares quite well on this measure of success. It is regarded highly by the editors and journal referees who make the decisions about publication.

The Committee has also made an assessment, necessarily subjective, as to whether the investigators who receive grants are among the very best in the profession. There is generally a high consensus in any scientific discipline as to which researchers are making the most important contributions, and this consensus is borne out by the frequency with which their work is cited by colleagues in the discipline. The Committee is very well impressed by the quality of the principal investigators of NSF projects.

In all programs, scientists of the first rank are well represented, and a majority of distinguished scientists

have received NSF support. Of the five U.S. winners of the Nobel Prize in economics, four have been NSF grantees. Also in economics, every winner of the John Bates Clark Award since 1960 has been supported by the Foundation, as have been six of the 11 most recent presidents of the American Economic Association. In anthropology, 41 percent of the American Viking Fund medalists have been supported by NSF, as has every winner of the A. D. Kidder Award in archeology since 1968. Similarly, six of the 11 recipients of the Sorokin Award for distinguished research in sociology and all of those receiving the Stouffer Award for significant contribution to sociological methodology have received NSF funding. Of 57 American psychologists recognized by the American Psychological Association's Distinguished Scientific Contribution Award, 43 have been Foundation grantees. Although not indicative of grantees' quality on the average, these numbers do show that the Foundation has supported researchers ranked highest by their peers. Many of these scientists were supported by NSF long before their work was nationally recognized.

The Committee also considered whether adequate opportunity is provided for able young investigators to launch their initial post-doctoral research projects. Because of the funding situation, it can be concluded that this is a serious problem. If the Foundation is to continue to support established scientists who consistently produce first-rate work, then with essentially stable or declining total funding, it will be able to fund a smaller and smaller fraction of the increasing numbers of young researchers who have been produced by the graduate schools in recent years. The Foundation has evidently dealt with this dilemma by discontinuing support of some productive researchers and by squeezing very hard on the budgets of those who were given continuing support. In spite of this effort to provide funds for new projects, there is evidence (see Appendix B) that fewer young researchers are able to get their projects funded, particularly in psychology and sociology, although perhaps not in economics. Meanwhile, the Foundation has had to forgo the support of some excellent research and is providing a decreasing fraction of the research budgets of even those scientists whose work it supports. As a result, the progress of research is slowed, and universities are, in effect, being asked to subsidize more and more heavily the NSF-supported research carried out by their faculty members.

In summary, on the basis of our familiarity with it and by all of the available measures, the quality of the

basic research in the social and behavioral sciences now supported by NSF is excellent and reflects many of the significant current emphases in the disciplines that are represented. The Foundation has a generally good reputation among research scientists for the reasonableness and fairness of its procedures and for its scientific judgment in selecting projects for support, at least with its current budgets and policies. At the same time, the Foundation turns down many good research projects and severely reduces the budgets of others in order to work within the limits of the funds allocated to these programs.

The Committee also rates the research programs very high in terms of the relevance and potential applicability of the products of the work to the problems of society. As in the physical and biological sciences, the directness of that relevance and applicability varies from field to field. But as we observed earlier, there has never been a very great distance between basic and applied concerns in the social sciences, nor is there a great distance in the research supported by the NSF programs.

Opportunities Lost

Our evaluation of the program in terms of what is not supported is less unqualifiedly positive. There are four broad deficiencies, all of them interrelated, in the present program: very few large-scale projects are supported; expectations of long-term support are discouraged; it appears difficult to obtain grants for major capital equipment; and there has been insufficient attention to the possible need for national facilities to carry on particularly expensive activities or to operate particularly expensive equipment.

Project Size. There is no official NSF policy with respect to project size, except for the requirement that grants of $500,000 or more must be approved by the National Science Board. At the same time, program managers and panels are reluctant to entertain proposals that would, individually, consume a major part of a program budget. Submission of such proposals has on some occasions been actively discouraged by reference to the size of total funds available, and there is by now a widespread belief among researchers that NSF is no place to go with a large project. Whether this belief is grounded in fact or not, it has a chilling effect upon the imagination: it does

not encourage large dreams, no matter how innovative those dreams might be or how significant the research goals they contemplate.

Not all dreams come true--nor should they. It is important, however, that the NSF be viewed by scientists as a place where they can go with important large-scale research proposals and where they can expect such proposals to receive careful consideration. If a proposal is meritorious but outside the scale of the current budget of a program, then it is the responsibility of the program manager and the division director to represent that proposal to the higher management of NSF and to present its claims for funding, so that its importance may be weighed against the importance of allocations to projects in other parts of the Foundation. It is not generally thought that large-scale proposals in the behavioral and social sciences now receive such representation or are given an adequate opportunity to compete for funds.

Long-term Support. Formally, project grants are awarded for periods of a year or two years. In fact, grants to projects of high quality are frequently renewed, and some outstanding investigators have had continuous support from the Foundation since its programs in their disciplines began. The Committee sees nothing in these formal arrangements that calls for change. However, a number of grantees or potential grantees have reported that members of the NSF staff have expressed the view that the Foundation should not be expected to support any line of research indefinitely; the fact that such research had been supported for some time might in itself be a reason for not renewing support.

We do not know whether such views reflect actual Foundation policy or whether they have, in fact, affected decisions to approve or reject particular proposals. We would regard such a policy as regrettable. Each proposal, whether new or for renewal, should be judged on its merits as science, without regard to how long activities in the same direction or of the same investigator have been supported. If a line of research has worn itself out and is no longer producing important new knowledge, then of course support should be discontinued--for the same reason that support should be withheld from a new project that does not show promise of yielding significant results. But duration of funding is itself no criterion for judging the merits of proposals.

Capital Funds. As the purchasing power of research funds has diminished in recent years and project budgets have become continually tighter, postponable items and those items that could be pushed off to the researchers' institutions have tended to be squeezed out of grant budgets (see Appendix C). Among the classes of items that have suffered are equipment, particularly large and expensive pieces of equipment, salaries for released faculty time, and the cost of computer rental. (In a later section of this report, we will mention several kinds of capital expenditure for which there does not now appear to be adequate provision in the program. It will probably be impracticable to meet these needs unless explicit provision is made for them in future budgets.)

National Facilities. In the physical sciences it has been found that certain kinds of important research facilities are too expensive to be duplicated on numerous university campuses. In order to provide such facilities to researchers in many locations, the institution of the national laboratory has evolved. The basic concept is that the scientific management of an expensive facility of this kind should be entrusted to a group of universities and that the facility should be available to all qualified scientists on a competitive basis. The applicability of this concept to certain facilities in the social and behavioral sciences, which we will describe in a subsequent section, should be given careful consideration. While the funding requirements for such facilities may not be as great as for some of the largest facilities in the physical sciences, they are well beyond the usual scale for NSF grants. The Committee recommends that attention be given to the desirability and feasibility of creating national facilities to meet the needs for research capabilities that cannot be provided on individual campuses.

Conclusion

The program of basic research in the behavioral and social science disciplines in NSF, as now organized and managed, does a very good job of selecting for support first-rate research projects of moderate size that fit within the traditional framework of those disciplines. The Foundation is to be commended for having established and maintained this high level of excellence during the initial period of support for those fields by the Foundation.

The next task of the Foundation, working with researchers, is to search out the important opportunities for the development of knowledge that cannot be seized effectively within the framework of the present policy, administration, and budget, but call for new imagination and new initiatives.

Emphases of Current Program

Prefatory to talking about possibilities for new initiatives in the Foundations's program, we wish to characterize and comment upon each of the current programs of basic research in the behavioral and social sciences. We continue to resist the temptation simply to substitute our own judgments of what research should be supported for the judgments of those who have made the decisions. To succumb to that temptation would violate one of the Committee's basic principles: that, in general, the collective wisdom of the scientists in a discipline is the best guide to determining what research directions are currently most promising and what investigators are ablest in that discipline. It is largely in new research areas, not represented in established programs, that the principle is suspect and requires modification.

In accordance with that principle, the following discussion of each of the existing programs for the support of basic research characterizes the current program emphases; comments upon changes in the field's direction that we see taking place at the present time or that we predict for the near future; and points out divergences, where we see them, between the Foundation's present program and the promising research directions perceived by the research community itself.

The recent reorganization of the Directorate for Biological, Behavioral, and Social Sciences has brought about a few changes in the list of behavioral and social science programs administered by two of the four divisions in that Directorate. (The other two divisions administer programs in the biological sciences.) The Division of Behavioral and Neural Sciences administers the new program in linguistics and the programs in psychobiology, neurobiology, social psychology, and anthropology. Some parts of the psychobiology program, as it is described below, and the neurobiology program are being transferred to the new sensory physiology and perception program and the new program in memory and cognition. Economics,

sociology, geography (renamed human geography and regional science), law and social science, political science, history and philosophy of science, and science policy remain in the Division of Social Sciences. The social indicators program continues to be administered through the special projects program.

Psychobiology

This program has supported comparative psychology and ethology as well as most of the experimental psychology (other than experimental social psychology). The striking fact about the program is its enormous diversity, for it covers not a single discipline or area, but many. When the proposals funded during the last fiscal year are classified roughly by area, almost one-half fall in the categories of ethology or comparative psychology, concerned with non-human species. Most of the others may be classified as human experimental psychology; even some of these employ animal subjects--rats or pigeons--but are aimed at a general psychological problem or process rather than species-specific behavior.

Of the human experimental projects, about one-third are concerned with some aspect of conditioning or stimulus-response learning theory. The remaining more cognitive topics include a substantial number in the area of psychophysics. Except for one project on illusions and one on attention, none appears to be concerned with perception, and there are none on emotion. There were three projects concerned with development (one non-human) and two rather large institutional ("coherent area") grants. Nearly all the infra-human projects concerned behavior, especially social behavior.

Among the projects that were declined during this same year, roughly the same number can be classified under infra-human behavior as under human experimental psychology. Of the latter, significant numbers were concerned with conditioning or learning, psychophysics, perception, emotion or affect, and development (two of these on aging). Some aspect of memory was a frequent topic of both the proposals accepted and those declined.

Within human experimental psychology, traditional topics like conditioning and psychophysics have received the bulk of the support. Part but not all of the balance in the current program may be explained by the relative numbers of applications received. However, more innovative

topics appear with somewhat greater frequency in the declinations than in the approvals. In any event, the pattern of support does not reflect accurately the current research interests in experimental psychology. Although the emphases of research supported by the National Institutes of Health (NIH) and the National Institute of Mental Health (NIMH) may somewhat compensate for the rather different focus of this program, that support has decreased significantly in the past several years as the result of pressures on funds in those agencies. At the same time, substantial programs of support for basic research in experimental psychology in the Department of Defense (the Office of Naval Research, the Air Force Office of Scientific Research, and the Office of Surgeon General in the Department of the Army) and the National Aeronautics and Space Administration have either been terminated or greatly curtailed.

In sum, the budget pressure on the psychobiology program is severe. In addition to the problem of total funding, the Committee is not persuaded that the overall balance corresponds to the present state of the disciplines involved, in particular, with the recent rapid growth of research on human cognition. Moreover, researchers in the newer areas have not been strongly represented on the program's advisory panel during recent years, so that the panels have not been particularly well constituted to evaluate projects reflecting the new trends.

The reorganization of the Division of Behavioral and Neural Sciences will separate new programs for sensory physiology and perception and for memory and cognition from the psychobiology and neurobiology programs. This step will go far toward correcting the difficulties in the present arrangement. Proposals in psychophysics will presumably be assigned to the sensory physiology and perception program, while the revamped psychobiology program will deal with behavior genetics, hormones and behavior, studies of non-human behavior, and other topics in physiological psychology.

In administering the new program of memory and cognition, human cognition will need to be interpreted broadly so as to include perception and attention, memory, thinking, and motivation as they affect cognition and learning. Developmental and cross-cultural studies of cognition should be encouraged. Problems of motivation that are oriented toward personality differences and social interaction should continue to be handled by the social psychology program, as should social-psychological topics in

child development. Panels will need to be created for the new programs with memberships spanning the appropriate range of subdisciplines.

Neurobiology

The neurobiology program has supported research in sensory physiology, which is now in a new program; neural processing; genetic, development, and molecular neurobiology; and behavioral neurobiology. Although it has close relations with psychology (about one-third of the research is behavioral), the neurobiology program lies primarily in the biological sciences. Hence, it lies outside the agenda of this Committee, and the scope or emphases of its projects are not reviewed in this report. We note, however, that budgetary limitations have apparently imposed severe restrictions on this program, and, as is the case in psychobiology, support for basic research in the neurosciences by other agencies has greatly declined.

Social Psychology

Social psychology studies the behavior of individuals in their social environment. In recent years, investigators in this field trained in psychology have conducted mainly laboratory studies, while many of those trained in sociology have worked on problems of personality and social structure, using data from sample surveys or non-laboratory research settings. The latter seek their research support from the sociology program or from NIMH. The NSF program reflects the importance a large segment of the field attaches to using laboratory experiments to resolve theoretical questions.

In the last year the program made 44 awards, more than half of them continuations of previous grants. Nearly one-half are studies of interpersonal and group interaction in which factors such as crowding, urban stress, cultural differences, and organizational setting are examined. An almost equal number of studies of attitude development and cognitive processes were also supported. The remaining grants were distributed over a number of topics. All projects ranged from those having mainly theoretical implications for social and cognitive processes to those bearing on applied problems in health, management, and education. The recent trend toward

studies in natural settings and of behavior over time is reflected in eight grants entirely for field or longitudinal research, while 14 more projects included plans for substantial field work.

The proposals that were declined were distributed by area of interest and type of research in much the same manner as those funded, except that a slightly larger proportion of the proposals for research on individual differences were declined. The investigators who were awarded funds had excellent reputations for high-quality work, as did a majority of the researchers whose proposals were rejected. Apparently, a substantial number of good proposals were rejected because of lack of funds.

Social psychology, recently subjected to much self-scrutiny, is ready for research with a broader focus, which requires longitudinal data collection and the observation of many variables at a time and which complements laboratory with field research. The proposals received by the Foundation and those recently funded do not yet reflect this new trend very strongly, although the program has been supporting a number of studies aimed at advancing field methodology. There is some evidence from an examination of projects declined that field studies have difficulty in competing, in the eyes of reviewers, with laboratory studies in the "hardness" of their methodology.

Anthropology

Anthropology is a highly diversified field. It studies the origin of the human species, the development of agriculture, and the rise of civilizations, contemporary tribal, peasant, and urban populations, and the people and institutions of industrial nations, including the United States. By its examination of the many forms of human existence, it frees the scholar from the limitations of knowing only one culture and from being trapped in a single tradition of history.

The Foundation's program in anthropology supports primarily prehistoric archaeology, human origins, the new field of primate studies, and social and cultural anthropology. Linguistic anthropology (now supported within the linguistics program), classical anthropology, and the archaeology of historic sites are not supported to any significant extent. These distinctions and exclusions correspond reasonably well with conventional definitions

of the discipline of anthropology as it is represented in university departments, except that the subdiscipline of applied anthropology is apparently absent from the 1975 grant list, although several of the rejected proposals were in that category.

Relative to the respective sizes of the subfields, the balance of support favors archaeology and physical anthropology. Of the 1975 grants, one-half were archaeological, one-third were in cultural or social anthropology, and the remainder were in physical anthropology. Examination of the rejected proposals shows that physical anthropology applications have a higher success rate than others. Cultural and social anthropology applications appear to be fewer than would be expected on the basis of the relative numbers of specialists. The subdisciplines most heavily supported probably have fewer alternatives to the NSF program for funding than do the others. In general, the program appears to be alert and responsive to new as well as conventional lines of research.

Funds provided by other agencies, such as NIMH, for the support of basic research in cultural and social anthropology are becoming increasingly tight. The anthropology program in NSF may expect that it will need to increase its support for these subdisciplines. To do this without impeding the imaginative research now being supported by the program will require an increase in the total program budget.

Linguistics

Linguistics has just attained program status, and the proposals funded so far were administered through the special projects program. The projects that are supported have a somewhat traditional focus, with a heavy emphasis on descriptive linguistics. About one-quarter of the projects involve study of some non-American language--more than are concerned with any aspect of sociolinguistics or psycholinguistics, even when these terms are interpreted liberally. Artificial intelligence approaches to linguistics are not well represented in the present program. The number of psycholinguistic projects that were declined did not appear to be out of proportion with those in other areas.

Linguistics is a very heterogeneous field, which has only recently become a separate discipline having its own departmental status at many universities. However, at the

present time, a large part of the linguistic research that is relevant to behavioral science finds its base outside linguistics departments. The last five years have seen rapid progress in describing the organization of human semantic memory, in analyzing the processes whereby language is interpreted and understood, and in programing automatic systems that have a considerable range of capabilities for handling natural language. There are close ties between this work and the main lines of contemporary research in cognitive psychology. A panel is needed for reviewing the proposals in this program, and the panel needs to be broadly constituted, representing psycholinguistics, sociolinguistics, computational linguistics, and artificial intelligence, as well as the core of traditional linguistics.

Economics

The economics program has stressed theory, quantitative methods, and data analysis in economics. It has supported research on: the development of complete and abstract models of the economy; the methodology and practice of estimating such models; quantitative analyses of the operation of labor and money markets; the development of models and data to understand our urban and energy problems; and many other topics. It has enlisted a high percentage of very distinguished investigators, many of them quite young, and it appears to have had a good track record of supporting much of the innovative research in economics.

While there is no difficulty in finding excellent projects in the areas of this program, budget stringencies and expectations of relatively short-term support have inhibited work on model testing as opposed to model development, and there are now also problems in finding funds for new directions: expectational economics (dealing with decision making under uncertainty and bordering on psychology), the economics of public choice, and the interaction of economic with political factors in dealing with such problems as inflation.

Progress in economics, both at the micro (individual behavior) and macro (aggregate) levels, is still very dependent upon the quality of the data available. Within the present budget of the program, there is little opportunity to fund projects for the development of better data and new kinds of data, or even to study the methodology of data collection.

Human Geography and Regional Science

Modern human geography has three sets of concerns: the distribution of events in space and the significance of this distribution for the operation of human institutions; the relations of people and nature; and the comparative analysis of foreign countries and cultures. At the interface of geography with regional economics and city and regional planning, a new discipline has emerged in some institutions, regional science. In addition, much geographic research is undertaken in schools of city and regional planning and in interdisciplinary centers for urban studies. The small NSF geography program has supported research in geography's subfield of spatial analysis, in regional science, and in planning.

Many of the projects have been methodological: attacking the difficult statistical problems of analyzing data on spatial distributions and the logical problems of developing sound urban and regional models. Substantive work has been supported on urban development and regional planning, and on the ways in which people perceive their social and physical environments and behave in light of these perceptions. However, most environmental research projects have been supported elsewhere in NSF, for example, by the special projects program and RANN's natural hazards program.

Law and Social Sciences

The program in law and social sciences is characterized by not falling within a single discipline and by having an obvious connection with a specific area of application: legal institutions. There is no sharp boundary between it and the corresponding program in RANN; whether a project is supported by one or the other is often a matter of negotiation between the program directors. The idea behind the program is that many questions about legal institutions could be illuminated by the right kinds of basic social science knowledge. Some of the investigators in this field are based in law schools, others in departments of sociology, anthropology, social psychology, or economics. The program does not now support any special mechanism for encouraging communication among this diverse group of researchers.

The present program is quite diversified, as might be expected in a field as heterogeneous as this one. One

important focus is the social psychology of the decision processes in juries, arbitrations, and other kinds of negotiations. A few of the projects appear to be indistinquishable from standard legal research, and a question may be raised as to whether their social science content is sufficient to justify their support by this program.

Like the other programs, this one needs an advisory panel, with representation from both the profession of law and the social sciences, for proposal review and for guidance of the development of the program.

Political Science

The program in political science is small ($1.5 million), both absolutely and in relation to the number of researchers in the discipline. Although some investigators and projects have received support for a number of years, continuity of support has not been a major feature of the program.

The program emphasizes studies of social processes (e.g., attitude formation, decision making, socialization, conflict, change); it does not emphasize studies of social institutions (e.g., political parties, bureaucracies, legislatures, chief executives, courts) or the historical or social context of political life. Almost none of the currently funded projects has a significant institutional or historical focus, while about one-sixth of the declinations appear to do so.

The program also emphasizes formal theory (e.g., social choice theory, control theory, spatial models, simulation). Of the grants funded in the most recent fiscal year, nearly one-third were mainly concerned with formal modeling, and others had a sizable formal component. Proposals involving formal modeling as an exclusive or primary concern were less well represented (not more than one-fifth) among the declined projects.

As a result of these emphases, the program funds a relatively large number of projects using theories or methods akin to those of other social sciences: in studies of social choice theory, there are close ties with welfare economics, while much of the research on political behavior is close to social psychology and sociology. The relations with the other disciplines are less conspicuous in studies of decision making and some features of international relations.

The program is selective in its support of the major fields of contemporary political science--public law and political philosophy being essentially excluded. The

program supports very little research in American national government, state and local government, or public administration. Its main foci are political behavior, international relations, and comparative government and politics. About one-half of the projects funded in the most recent fiscal year deal with political behavior; international relations and comparative studies each account for about one-fourth.

Relatively few proposals in American national government or public administration are received by the program. Among the other fields, success ratios are highest in international relations, next highest in political behavior, and lowest in state and local government.

The program provides little current support for two kinds of work that have, in the past, made important contributions to the study of politics. The first is the development, maintenance, and utilization of major data resources, including large-scale sample surveys, archives of individual and governmental actions, and biographical material on political actors. The second is the close analysis of political specifics: the explication and interpretation of historical events, the ethnographic analysis of institutions, the textual analysis of records, and the clinical analysis of individuals.

Sociology

Historically, the sociology program has paid a good deal of attention to increasing the rigor of research methods and of theorizing, finding new methodologies for analyzing data, and improving the quality of data. This is still a major focus of the program. On the substantive side, the emphasis is currently on the relation between social structure and individual behavior (which is what most sociologically trained social psychologists study), complex organizations, social stratification and social mobility, social change, and demography and ecology. The program staff is interested in promoting further activity in historical demography and the interaction between society and the environment.

Research in sociology could benefit greatly from the availability of longitudinal data for important social and social-psychological variables on large and representative samples of individuals. Currently, there are relatively few longitudinal studies of national samples with satisfactory retention rates over any long period. That it is

possible to establish and maintain such data files has been demonstrated: one study succeeded in following a random sample of 10,000 persons over a 17-year period with a retention rate of approximately 90 percent.

Projects undertaking to develop such data are quite expensive and must be funded over long periods of time. They encounter many vexing problems, including the difficulties of maintaining high response rates, the need to select periodically new panels to be followed so that trend analysis will be possible, and the problems of privacy that are raised when retaining data on individuals over a long time period. The special projects program has just funded a pilot study, "Assessment of Survey Practices and Data Quality in Scientific Surveys of Human Populations," under the aegis of the American Statistical Association, that may address some of these problems.

There is need also for long-term funding of periodic sample surveys of the opinions, concerns, aspirations, and achievements of the American population, such as the general social survey-national data program of the National Opinion Research Center (NORC) funded by NSF, and the replicated study of occupational changes in a generation funded by NSF-RANN. Another source of data of great value for sociological research would be provided by developing public use samples of earlier censuses and Current Population Surveys. (This proposal is developed further in the section on new opportunities and developments.)

Special Projects

Until recently, linguistics made up a large part of this program. It is a useful program to serve as a spawning ground for new programs, like that one, and to deal with projects of a distinctly interdisciplinary character. A program of this kind does pose special management problems, however. Because it has no single reference discipline, there may be serious problems in obtaining suitable and reliable evaluations for projects and in assigning relative priorities to proposals that are nearly incommensurable. Under these circumstances, there is always a temptation to pick up topical areas whose longer-term prospects may not be very attractive. On the whole, the program appears to have avoided that temptation in the past.

Three new special projects are now under development or consideration. One is an assessment of survey practices and data quality in scientific surveys of human populations,

referred to earlier, of which the pilot study has now been funded. The second is a program of research on environmental perception, planning for which has been going forward through a series of workshops. The third is a proposal for a program of special emphasis in mathematical social science, which, if funded, would carry out some of the functions of the Mathematical Social Science Board, but with some rebalancing of effort. It would generate direct research and studies with an emphasis on mathematical and computer simulation tools and methodologies, as well as continue to support conferences and workshops in mathematical applications.

Social Indicators

Social indicators is a budgetary program within special projects. This program, like those on science policy and on law and social sciences, cuts across the established social science disciplines, involving especially sociology, social psychology, and economics. The program is more substantively focused than the disciplinary programs. These differences in structure and orientation raise at least two sorts of issues for the program: the nature of the panels and disciplinary reference groups that can be used for program planning; and the relation between basic and applied research objectives.

To aid in program planning and as a means of communication among scientists from all the disciplines concerned with the program, NSF has provided funds to the Social Science Research Council for coordinating the research on social indicators. This mechanism provides a scientific reference group for researchers that can perform many of the functions performed by the established disciplines for their programs.

The social indicators program is an excellent example of how basic research in the social and behavioral sciences can be brought to bear upon topics of great social significance, such as environmental quality, family stability, and education. There is good communication between the investigators and those who are concerned with the application of social indicators to public policy matters. Research support has been provided not only to investigators in universities, but also, on a substantial scale, to such organizations as the Urban Institute, the National Opinion Research Center, the Institute for Social Research, the National Bureau of Economic Research, the National

Planning Association, and the Bureau of Social Science Research.

History and Philosophy of Science

The history and philosophy of science program supports work in three fields--history of science, philosophy of science, and sociology of science--with a heavy concentration in the first-named. Work on the sociology of science is supported jointly by this program and the science policy and sociology programs.

The program supports a small number of projects, almost all headed by well-known investigators, in the philosophy of science. Most of these projects focus on questions of scientific methodology. A much larger number of projects is supported in the history of science, concentrating in the physical sciences. There does not appear to be a particular emphasis upon the social history or sociology of science, its relation to science policy, technology, or society in general; instead, most studies focus on particular scientists or on understanding particular lines of scientific development.

Science Policy

The present pattern of activities supported by this small program is generally sociological in character, with a strong quantitative emphasis. Substantial resources have been devoted to exploring the uses of citation data.

New Opportunities and Directions

In suggesting that there are major needs and opportunities, not now being met, for supporting large-scale and long-term research, the Committee is not proposing that the Foundation depart from its basic practice of supporting the best-conceived, best-justified projects that come to it as unsolicited proposals. On the contrary, we believe that a considerable volume of good proposals for large-scale and long-term research would come to the Foundation if it were perceived as interested in entertaining them. It would be inappropriate, therefore, for us to construct our own fixed agenda of priorities and needs; but it may be quite appropriate to give some examples of areas that have

important needs for research support and that could make significant contributions to knowledge. We have not tried to be exhaustive in our list, but have sought to select examples that will illustrate the variety of opportunities for new initiatives and some of the forms that projects might take.

Some of the examples are developed at greater length than others, either because of the inherent complexity of the topic or because several similar proposals are discussed together. In any case, it is the Committee's intent to identify these alternatives as a sample from a much larger population, and not to rank them, explicitly or implicitly, in their order of merit.

Several of the kinds of research we shall discuss are already being supported to some extent by the Foundation. In these cases the new opportunity depends upon support on a larger scale, with greater assurance of continuity, or under new administrative arrangements.

The Data Base: Economic Behavior

Collecting data for use in building and testing theories is an essential but not usually glamorous part of scientific research. It is generally felt, and reflected in the long-range plans of several of the social science programs in NSF, that deficiencies in the available base of social science data are seriously impeding the progress of research. In this section and the next two, we will outline some specific examples of research areas for which support for systematic data collection would have great value.

Among the social sciences, economics perhaps has available one of the most extensive bodies of data, most of them obtained as by-products of information gathered for the administrative purposes of government. But even in economics, which has access to these public records and government statistics, we clearly need better data on the condition, behavior, knowledge, and expectations of individuals and households. We need such data to achieve a better understanding of how basic consumer decisions that determine the demand for commodities and services in society are made.

Most important of all, data is needed that will allow the study of changes over time. Longitudinal data can test whether findings obtained at one time can be generalized to others, and we can investigate the processes of

change in society without relying on people's fallible memories of the past.

Some sequential records of individual or business firm behavior are now available, but the separate files are usually too narrow for the purposes of research, and concerns with privacy make it almost impossible to merge information from several sources. Most often, the data that are needed to understand the decision process must be obtained by interviewing individuals who speak for families, firms, or government agencies.

To be most useful, such data bases require repeated interviews with the same individuals or at the same sites, using reproducible methods and allowing reexamination of the earlier protocols to ensure constant scales of measurement. Studies to obtain data of these kinds require long-range planning and funding, stable organizations, personnel, and archives. A number of longitudinal data-gathering projects are going on currently--all of them, except for some government programs, operating with annual and unsure funding. And most of the government programs are heavily focused on measuring and monitoring ongoing administrative programs, rather than on providing data for research aimed at understanding underlying processes. Hence, it is usually difficult to use the government statistics as panel data for secondary analysis. Further, in governmental data-gathering operations, it is difficult or impossible to include information on such matters as attitudes about government, laws, taxes, race or religion, or to attempt measurements of motivation or personality.

Useful longitudinal data can also be obtained by repeated probability sampling of a population, using the same measuring instruments in each survey; this is less expensive to gather than panel data. Changes in identifiable subgroups, if not in identical individuals, can be studied. The protection of privacy is easier than with panel studies, since identifying information need not be kept to allow revisits. Although sampling variability of estimates of change is larger, it can be kept within bounds by revisiting the same sample points, or even the same addresses, repeatedly.

Whatever the method used, the development and preservation of longitudinal data require stable research institutions, at least some of them outside the federal government. Most of the capital cost of such facilities is a rapidly depreciating investment in trained staff, computer software, and sample information, so that stability depends much more on a steady flow of work than on

capital grants. It is preferable for such facilities to be connected with universities so that the new information filters rapidly into the curriculum and into theoretical work, and so that theoretical developments influence the design of the survey work. Several such institutions now exist: for example, the Institute for Social Research and the National Opinion Research Center. They carry on a wide variety of survey research in both economics and other social sciences. At present, these institutions suffer from serious budgetary insecurity because of their heavy dependence on short-term research support, and this insecurity creates severe problems for their status and the status of their staffs within the university.

The quality of the work done by data-gathering institutions depends also on their making continuing improvements in methodology. This too requires long-term funding beyond support for individual substantive research projects. The methodological work is best carried on, however, in close proximity to ongoing empirical studies. A major methodological concern is upward or downward bias in the responses of persons interviewed, which can jeopardize the entire data-gathering effort. Because of doubts about the validity of responses, almost no data have been gathered for a decade and a half on the distribution of assets, asset portfolios, or people's expectations and plans for the disposition of assets. During a decade of inflation and erratic shifts in the values of assets, there has been very little monitoring of what appeared in 1965 to be a trend toward planned and voluntary early retirement. These are just isolated examples of possible important changes in society for which there are few or no data. Intensive basic research on the motivations of respondents to cooperate with such inquiries is perhaps the best route toward improving both the response rates and the quality of the information elicited.

When we turn to archives, the potential data base includes not only survey data but vast collections of statistics from official records. NSF has already made large investments in archives of election results and congressional voting data managed by the Interuniversity Consortium for Political Studies. Some similar efforts in economics and demography are in order. With the smaller, more local or special, samples of much sociology and social psychology, it is more difficult to determine what should be archived, but the potential contribution of appropriate archives to sociological research deserves exploration.

The support of longitudinal, data-gathering activities calls for attention to administrative mechanisms and evaluative procedures. We have referred to the funding problems of existing institutions capable of doing this kind of work and their need for greater stability of support. Reasonable stability could be provided if, while still supporting research on a project-by-project basis and using the usual review mechanisms, the Foundation adopted a longer time horizon in the funding of specific research projects, giving them assurance of support for at least two or three years or even longer for longitudinal projects whose value depends on data collection over substantial periods.

Any discussion of the improvement of data bases necessarily raises the question of privacy. There is no single formula for protecting privacy in data collection; detailed procedures must, and can, be worked out in each instance. Over the past decade, we have had a great deal of experience with the technical and institutional means for reconciling privacy with data collection. It is important to point out that, while the problems of confidentiality of social science data are genuine (in the face, for examples, of the subpoena power of the courts), the serious invasions of privacy that have actually occurred in our society in the past few years have derived from quite other sources and causes than the collection of data. The record of experience shows that privacy can be protected without depriving society of the important kinds of data it needs about its own operation.

Data-gathering projects cannot be evaluated and reviewed, as is largely the practice with other basic research projects, on the basis of publication in books and journals. If continuing projects with long gestation periods are to be funded, provision should be made for periodic review and assessment, by appropriate panels, of the quality of the data-gathering effort and the importance of the uses, present and potential, of the data.

The Data Base: Public Use Samples

The social and economic transformations experienced by American society in the last generation can be traced roughly from published census data for 1940 through 1970. However, for a more thorough understanding of the dynamics of social change, analysis of the massive files of the detailed data is necessary. While a part of this need has been met by creating the Public Use Samples of the 1960

and 1970 censuses, these samples provide data for only the most recent periods. The 1940 and 1950 censuses constitute rich but currently unexploitable sources of data that could permit the analysis, for example, of trends in fertility patterns, labor force participation, income distribution, geographic migration, educational and occupational attainment, patterns of family formation and dissolution, and differences in these according to race, ethnicity, region, and other subgroupings.

The retrieval of the 1940 and 1950 census records and the production of Public Use Samples of these early census materials is an extremely demanding and costly undertaking that no government agency has been able to finance, despite the acknowledged need for these samples for research in sociology, demography, economics, and history, as well as for the work of several government agencies. Public Use Samples covering the four censuses, 1940-70, would be of enormous value in basic and policy-oriented research in the social sciences, and the knowledge and technology required to retrieve the data from the earlier censuses are available. To produce the new samples, however, would require funds on a substantial scale, perhaps as much as $7 million.

There is similar need to retrieve as much data as are still extant from the archives of the Current Population Surveys. Although basically a vehicle for assessing the levels of unemployment, these very large sample surveys, which have been taken on a monthly basis since 1940, have dealt with many other important topics over the years and represent a potentially important resource for the construction of social indicator models as well as for more detailed study of social change. Again, it would take a major effort to retrieve the information from the original files and put the data in an appropriate form for public use. In addition, it would probably be necessary to design and support a facility to manage the resulting data files and to carry out complex data analysis. We believe a substantial investment of funds would be justified in order to recover this important additional data resource, currently unexploitable, for basic and policy-oriented social science research.

The Data Base: Other Examples

Another data-base program would be the study of "index communities." A small number (less than two dozen) of

American communities could be studied both historically and anthropologically over an extended period of time. Although the generation of a representative set of communities would be difficult, the procedure would produce data amenable to many scientific uses and would provide a way of coming to understand how a variety of social and cultural trends are articulated in the lives of community members.

In the discussion of the data base for economics, the role of survey research institutions was mentioned. These institutions are also important, of course, in building the data bases for sociology and political science. They provide one of the principal means for obtaining the information we need for the examination of social change.

In individual psychology, longitudinal studies of the growth and change in human abilities, attitudes, and other characteristics over extended time have been rare. Terman's monumental study of a group of gifted children, who were reinterviewed periodically until well into their 50s, is one of the few such studies that have been carried out. Most research in child development has not been concerned with tracking the growth of particular children, but with comparing groups of children at different age levels. We know very little about human long-term memory because the laboratory studies of memory carried out by psychologists seldom test retention for more than a few weeks (and more typically, for only a few hours). Longitudinal studies in individual psychology have been impeded by the same conditions that discourage such studies in the other social and behavioral sciences: the absence of a stable institutional and funding environment that would enable such studies to be carried to completion.

Comparative Research

Cross-cultural studies are of importance to research not only in anthropology, but also in sociology, linguistics, political science, psychology, and economics. Some of the basic research questions about human societies require a comparative approach to avoid a parochialism that overgeneralizes from data gathered in a single culture (usually one's own).

In the development of programs of geographical area studies in the social sciences, particularly for the nonwestern world, so much emphasis has been placed upon developing expertise in the economy, polity, society, and

culture of a given nation that explicit comparison with other national experiences has been neglected. At the same time, investigators concerned with the comparison of cultures have tended to use data from various nations and sources, sometimes without deep understanding of the specific deficiencies of those data or of the different institutional factors of the various countries they are comparing.

The problems that social scientists have been encountering in keeping their welcome in host countries, the fiscal limits at NSF, and the increasingly mission-oriented character of NIMH research have all contributed to a reduction in research on foreign societies; expensive cross-cultural research, which attempts to coordinate studies in several countries, has perhaps been hit hardest of all.

Today, comparative research undertakings both should and must usually be undertaken collaboratively by scientists from the several societies being studied. When meritorious projects are proposed, it would be desirable if NSF would use its offices or those of the Foreign Secretary of the National Academy of Sciences to assist in negotiating joint sponsorship by the several nations involved.

The fact that the NSF program of research support is organized on a disciplinary rather than an area basis facilitates the review and funding of comparative studies, provided that unnecessarily sharp boundaries are not drawn between political, economic, sociological, and anthropological phenomena. It appears that adequate support of comparative studies could be accomplished with the present organization of programs if funds were available for the large-scale support that is needed.

Interdisciplinary Workshops

The Foundation could also play an important role in stimulating exchanges on common methodological problems among social scientists in different disciplines. Often such problems involve statistical inference from nonexperimental data, which appear in different guises in sociology, political science, or economics, and assume particular importance in studies using data from large numbers of individuals. Nonresponse in surveys is one such problem. Support of workshops or seminars bringing together social scientists and statisticians to examine such common

data-handling and data-analysis problems could make a major contribution to the research competences of investigators and disciplines. The Foundation has already had some successful experience with funding such activities, for example, through its support of the programs of the Mathematical Social Science Board and the seminars of the National Bureau of Economic Research.

National or Regional Facilities

Another class of new opportunities identified by the Committee is national facilities requiring expensive equipment that can only reasonably be provided at one or a few university campuses or other settings. In this section, we will describe several important facilities that illustrate this kind of opportunity.

Computer Facility for Cognition. There has been spectacular growth during the past decade in studies of human cognition that view the topic from an information-processing point of view, and great progress has been made in understanding human problem solving and other complex human processes in these terms. One of the important methods used in this research is simulation of human cognitive processes on large computers. At the cutting edge of the research today are computer programs that simulate the way people extract meanings from language and the way they store large bodies of information in memory so that it is available to help carry out professional problem-solving and decision-making tasks. By examining the structures of these programs and their performance in the face of difficult problems, we learn how people perform these same tasks and, through that knowledge, obtain new ideas for increasing human effectiveness in problem solving, learning, and decision making.

The principal source of support for computer simulation of cognitive processes has been the Advanced Research Projects Agency in the Department of Defense, but that agency is now more and more emphasizing applications rather than basic research. Since the physical facilities--computers and their auxiliary hardware--needed for advanced research on computer simulation are exceedingly costly, it is questionable whether they can or should be duplicated on many university campuses. (There are now perhaps four university centers, one laboratory in a non-profit organization, and two corporate laboratories more

or less equipped for such research; all of them are limited by their hardware, especially by memory size.) A national facility, which could be used by scientists at a number of institutions communicating largely through a telephone network, could probably be established with a capital expenditure of about $10 million.

As a preliminary to creating such a facility, an exploratory planning study is needed to investigate its feasibility and desirability. Such a facility should have a scientific program directed by an inter-university committee of scientists, should be open to scientists on a competitive basis for their investigations, and should focus its activities especially on simulation in cognitive psychology.

Laboratory Facilities for Psychoacoustics. There is a need for a national laboratory, or several regional laboratories, for psychoacoustical research, to provide a setting in which very precise stimulus control is possible. The basic stimulus generation process for sounds is relatively simple and not excessively expensive; what is more costly is the equipment needed to deliver the stimulus to the eardrum over a wide frequency-intensity range, with good signal-to-noise ratio even at the lowest intensities. The proposed laboratory would be unique in its ability to generate and measure acoustic stimuli accurately over the entire audio range.

From the viewpoint of total support in the psychoacoustics field, the existence of a national or regional facility would reduce the need for large capital investment by many individual researchers. Research in psychoacoustics has continued to expand over the past decade, and there appears to be a continuing need for the kinds of information such a laboratory would produce, especially as it bears on environmental questions such as noise pollution. The initial investment in such a laboratory, excluding the cost of the building, might be $500,000; an annual budget of about $250,000 would be necessary to staff it and conduct the experiments.

Anthropological Museums. There are a substantial number of museums in this country containing valuable archaeological and ethnological collections. In many cases, these institutions are inadequately financed, lack the libraries required by investigators working with their materials, and lack the temperature and humidity control and equipment necessary to preserve the collections in

good condition over any lengthy period of time. A program of grants would gradually upgrade the quality of these facilities for the better preservation of their collections and help make them usable working laboratories for local and visiting investigators.

Interdisciplinary Centers or Institutes for Advanced Study. There is a need for continuing long-term support for one or more centers where groups can work for several months or a year on common problems with complementary skills, or where individual scholars can pursue their writing, theorizing, planning, and reflection for a sustained period free from interruption, and under circumstances that facilitate scholarship.

Residence for a limited period at such a center would permit an investigator to devote full time to scientific activities. Particularly in the behavioral sciences, where there has not been a strong tradition of post-doctoral study, such centers can play a vital role, both for the young and the mature scholar, in the completion of major research undertakings, in the reassessment of old research commitments and priorities, and in the exploration of new problems and paths of inquiry.

A facility for 45 to 50 scientists can be operated at a total annual cost of roughly $2.5 to $3 million. With annual funding at a level of about one-third of that total operating cost, the National Science Foundation could provide for the long-term continuity of such an institution, which could be expected to obtain the rest of its support from other agencies and foundations and from fellowship grants to individual investigators.

Other Facilities. A number of other possibilities may be mentioned as candidates for new national or regional laboratories or for the stabilization and expansion of facilities of these kinds that already exist. Among these are laboratories for the maintenance of animals of known genetic strains or known experience, laboratories for simulation of economic behavior at the individual level (micro-simulation), and laboratories for the study of vision.

Again, the Committee is not in a position to recommend which, if any, of these should be developed; a careful preliminary study would be needed in each instance to determine need and feasibility. We mention them as examples of the kinds of projects that are now infrequently proposed to the Foundation and that we believe would be proposed more frequently if it were known

that the Foundation could and would entertain such projects. Today, many if not most behavioral and social scientists picture the Foundation as a possible source for research funds on the scale of $40-50,000 per year for projects of one or two years' duration. We are suggesting that only by changing that picture will the Foundation discover the nature and magnitude of the real national research needs in those sciences.

Management of Basic Research

Our comments on the individual basic research programs raise a number of questions about the management process. Some of these questions may well apply to the whole range of NSF programs, but we will discuss them only in their application to the social and behavioral sciences.

Proposal Review Panels

The first and perhaps most important of these questions has to do with the use and role of panels and mail reviewers in the proposal review process. There are substantial differences among the programs in the ways in which panels are used. The psychobiology program operates a panel in much the same way as NIH and NIMH do: each panel member rates each proposal according to individual judgment of its merits, and then the ratings are averaged to provide an overall ranking of projects. Although the program director is not bound to follow that ranking, it has considerable influence on the final selections. Apparently, the rating of projects is not common in NSF, and no other programs in the social and behavioral sciences follow this practice. In the other programs, the program director receives individual project evaluations from panel members and hears the panel discussion, but does not necessarily determine an average rating that results in a ranking of all the projects. In six programs, panels are not used at all; reviews are carried out by mail reviewers, and sometimes by ad hoc site visits. These six programs are Human Geography and Regional Science, Law and Social Sciences, Linguistics, Special Projects, Social Indicators, and Science Policy.

Mail reviewers are generally sent individual project proposals, which they then evaluate on an absolute basis without being able to compare them with other current

proposals. This increases the difficulty of comparing evaluations among reviewers and probably reduces the overall usefulness of the reviews for ranking the projects in order of merit. Evaluations arrived at by members of a panel after opportunities for discussion are undoubtedly more reliable as a basis for deciding the relative merits of projects. Moreover, we believe that program directors are likely to give greater weight to judgments reached in panel meetings and that it is desirable that they do so. We recommend strongly that panels be instituted for those programs that do not now have them and that panels be asked in all cases to rate proposals.

In the Committee's judgment, the present review practices, and in particular the absence of ratings by either panels or individual reviewers, make the program director's job more difficult than it should or need be. In those cases in which programs coincide more or less with disciplines, it is important that the program directors have reliable ways of learning the sense of the disciplines with respect to research directions and the quality of proposals. Even if the program directors consult extensively with members of the discipline, there is always the danger that they will inadvertently hear what they want to hear, particularly if opinions are not unanimous. Ratings provide information that is not subject to that kind of misinterpretation.

We are not implying that program directors should be bound by average panel ratings in their final recommendations, but we recommend that they should determine rankings, consider them, and explain their deviations from them. Program directors should also report back to the panels on the disposition of the proposals they have reviewed. We believe that program quality is generally higher when this kind of assistance is available to program directors, and when they use it.

Further, we recommend that, whenever possible, mail reviewers should be sent more than one project proposal at a time to review. This need not increase the total reviewing work load, but simply impose it on reviewers in a somewhat lumpier way. We believe that most reviewers would prefer this to reviewing individual proposals without some information about the alternatives.

Panels need to be selected to be broadly representative of the disciplines covered by the programs. This is easy to accomplish, and has generally been accomplished, when a program corresponds to a single established discipline like economics or political science. It requires

greater care when this is not the case, for example, social indicators, special programs, or history and philosophy of science. In these instances there needs to be broad consultation, making use of mail reviewers and perhaps ad hoc panels, so that the programs will not simply reflect a single person's view of the field; no matter how gifted one individual may be, no one can adequately keep abreast of several disciplines.

Program Planning

Even though research programs in basic science should respond primarily to the trends revealed by unsolicited proposals, program directors have a significant review and planning function to perform in connection with budget presentations and discussions of future funding. They can perform this function adequately only if they receive appropriate help from researchers in the relevant disciplines.

The Foundation needs to experiment with means for providing that help. Possibly each review panel should schedule one meeting each year devoted entirely to overall program review, without distraction by the review of individual proposals. Another possibility, which we think may even be preferable, would be to convene periodically an ad hoc committee, including some former panel members, or asking some outside body, like the Assembly of Behavioral and Social Sciences of the National Research Council or the Social Science Research Council, to convene such a committee.

Some of the programs have made effective use of ad hoc workshops to explore new topics that seemed promising for development. One function of such workshops is to advise program directors with respect to possible programs on new topics or to the possibilities for new interdisciplinary programs. Another important function of workshops is to provide an opportunity for some of the best minds in a field or a group of related fields to explore the research potential of a new topic. This is often an effective means for exposing first-rate investigators to promising research opportunities and securing rapid deployment of research talent in important new directions. The usefulness of workshops of this kind depends heavily upon searching broadly in advance for the participants who are best informed and can offer the best ideas and advice, and program directors need to consult widely outside the Foundation to identify these persons.

The present structure of programs covers the main areas of the social and behavioral sciences in a reasonable fashion. There is always a danger, however, that research topics that fall between the disciplines, or that lie partly outside the social sciences, will not receive proper attention. The Division of Social Sciences has been alert to that danger and has used its Special Projects program to identify such areas and to undertake initial activity in them. We understand that the research support in geography, law and social sciences, linguistics, and social indicators began in this way before those subjects were elevated to the status of independent programs.

The quality of planning in the Foundation, as well as the competence with which individual proposals are reviewed, could be enhanced significantly if the program staff had periodic opportunities for reimmersion in the scientific community and for close contact with contemporary research developments in their areas of program responsibility. One excellent way to accomplish this would be to establish a sabbatical system that would permit staff members to spend six months or more in one or several high quality research settings related to their programs.

NSF Staff as Program Representatives

In the previous section of this report, a number of promising possibilities for new Foundation activities were mentioned. Some of these were singled out because they do not seem to fall neatly within the boundaries of the existing programs. Others call for novel forms of organization or novel facilities or for support with a degree of continuity and on a scale that is not typical of existing programs.

We have already observed that the present austerity of the NSF budget for basic social and behavioral science research does not encourage investigators to propose large data-gathering projects, or large projects of any kind for that matter, no matter how great the potential importance of the undertakings. Moreover, when such proposals are made (proposals, say, ranging from $250,000 upward), they are frequently rejected, not for lack of scientific merit, but because they would absorb such a large part of a program's budget that not enough would be left for other, smaller projects. In our review of meritorious proposals that had not been funded, we found a substantial number of

examples of this kind--perhaps as many as two or three per program per year. In our judgment, the relative dearth of funds to support large projects in the social and behavioral sciences, the reluctance of the Foundation staff aggressively to seek funds for them, and the reluctance of panels to support them exert a serious damping effect upon the imaginations and aspirations of researchers in these fields to investigate truly fundamental problems that are not approachable on a small scale.

The NSF staff managing basic research in the behavioral and social sciences has four main decision-making responsibilities. The three most obvious are long-range planning, budget recommendations and budget allocations among the programs, and approval or rejection of specific project proposals. The fourth, less obvious but no less important, is to represent the social and behavioral sciences at the higher administrative levels of the Foundation and in the Foundation's negotiations with the administration and Congress. This is a difficult assignment because the social and behavioral sciences are not well understood among lay people or among natural scientists; it is crucial that such understanding be enhanced so that policy decisions may be well informed.

In the Committee's judgment, this function has not been performed effectively. The level of appreciation for and understanding of the social sciences within the Foundation, but outside the social science programs themselves, is not impressive.

We have examined a number of documents prepared by the Division of Social Sciences to describe its work to a broader audience--for example, the budget submission to the appropriations committees of Congress. In our judgment, the budget document did not explain effectively, in lay language, what knowledge was being produced by the research that was supported or the significance of that knowledge for society. The introductory examples of "recent significant achievements" were well chosen, but in general the presentation was too abstract and was deficient in convincing, concrete examples of important research results. A sentence like "The properties of different allocative and signalling devices for distributing scarce resources over space and time provide a logical springboard for studies on the microeconomics of exhaustible resources" is likely to reinforce beliefs, already widely held, that social science is full of confusing or vague jargon. A statement like "A number of different investigators studying the processes of political decision making

in a number of different contexts . . . have identified certain constant features" makes vague and trivial what are perhaps significant findings. We encountered too many examples of these kinds of obscure and bland statements in the document and too few descriptions of significant and valuable research products.

There needs to be, in that and other documents, more explicit attention to the specific ways in which the social and behavioral sciences can illuminate with objective information questions that themselves are laden with values: how research can, for example, contribute to our understanding of the American party system and suggest how its operation could be improved, without that research taking a partisan position or offering a partisan advantage to one group or another. There needs to be more attention, also, to the measures that social and behavioral scientists have been taking to protect the privacy of their subjects and respondents, and in general to protect individuals against being harmed by their participation in research. These are reasonable issues for members of the public and for Congress to raise (and they will raise them, whether one thinks them reasonable or not); all of us as scientists, and the Foundation in particular, need to devote thought and energy to provide both reasonable solutions to the underlying problems, and convincing explanations of those solutions. This is a responsibility, not just of the Directorate for Biological, Behavioral, and Social Sciences, but of the entire Foundation as the organization most broadly concerned with the health of scientific research in the United States.

The same comments apply to the responses to Congressional questioning in authorization and appropriation hearings. The recent Congressional attack on "silly titles" for research projects again illustrates that the Foundation has not done a successful and convincing job of communication. The social and behavioral sciences will not get the legislative and budget support they deserve and need until this communication problem is solved. Solving it is a major administrative responsibility of the Foundation that does not appear to have received the high priority that it requires.

Responsibility for communicating more effectively about the content and significance of the research programs does not rest on the Foundation alone, but is shared by the research community. The Foundation can and should require from recipients of its research grants meaningful reports on the results of their investigations.

In addition to scientific reports directed to their fellow specialists, it is not unreasonable to expect accounts, written in language that is intelligible to lay people, of the objects of their research, the nature of their findings, and the meaning of those findings for broader social and human concerns. Writing good accounts of this kind is hard work, which takes scarce time from research. Nevertheless, we think most scientists will accept this responsibility if they believe that those accounts will be useful in communicating to others their sense of the excitement and significance of their work.

APPLIED RESEARCH

In the behavioral and social sciences, as we have already observed, the line between basic research and applied research is very indistinct. A substantial part of all behavioral and social research has rather direct implications for application to human and social problems, especially in enhancing our ability to evaluate the social and human consequences of technical or social innovations. Nevertheless, the creation of the applied research programs in the Foundation has encouraged some excellent investigators to reorient their work in an applied direction and has provided means of funding research in some important applied fields that do not fit the structure of the basic research program or the existing programs of federal mission agencies.

The National Science Foundation's commitment to applied research can be traced to the late 1960s when Congress expressed its desire that the Foundation selectively support scientific research relevant to national problems. The Foundation was viewed as an appropriate sponsor of these activities in fields such as social science and engineering, in which the categories of basic and applied research overlap and the opportunity to pursue research applicable to important social problems often arises as a logical extension of basic research. The Foundation's position as an independent science agency also made it an appropriate sponsor of basic and applied research important to national goals that did not fit under existing mission agency programs. An amendment to the National Science Foundation Act in 1968, together with Congressional committee reports and a letter to the Foundation's director from the chairman of the House Committee on Science and Astronautics, authorized and made explicit these intended functions.

The Foundation's support for applied research since 1971 has been administered largely through the program for Research Applied to National Needs (RANN) in the Directorate for Research Applications. While the short-term goals of this program and its criteria for project support have varied, the Committee believes the basic functions of the Foundation's role in applied research remain as important and proper as when they were formulated by the Congress eight years ago.

Social and Behavioral Science in RANN

Two distinct types of applied behavioral and social science research are supported in RANN. First, there is research relating to social phenomena and social institutions, which is almost wholly within the social sciences. An example is research on the institutional barriers to job opportunity and mobility, one of the elements in the Public Policy and Economic Productivity Program.

Second, there is research that has natural science and social science components, of which there are two kinds: one involves primarily economics-based cost-benefit analysis of technologies; the other involves evaluation research using social science data and techniques. An example of the first would be a study of environmentally sound and economically feasible methods for managing western range lands. An example of the second would be the evaluation of a communication system designed to link citizens more closely with their local governments, based on the use of telephones and computers.

Those two types of research tasks find modest support in RANN, even in relation to the total funds expended by the federal government for applied social research (a generous estimate would be about $300 million or $1.50 per capita). The annual budget for the Directorate for Research Applications in NSF is approximately $80 million; the part of this budget that supports behavioral and social science research is not easy to estimate, but was probably about $18 million in fiscal 1975. Thus, while the ratio of funds for applied research to funds for basic research within the Foundation is much larger for the behavioral and social sciences than it is for the physical and biological sciences--about 2:1 compared to about 6:1--the total sum is quite small when considered in terms of the tasks attempted and opportunities available.

The Committee has some rather substantial reservations about the effectiveness of the Foundation's current program of support for applied research related to the behavioral and social sciences. We believe that the basic objectives of the program--to support research that is directed toward producing knowledge applicable to the solution or amelioration of important problems in society--have been too narrowly interpreted in practice and that these objectives can be better accomplished by making certain organizational and procedural changes. The following pages set forth our evaluation and recommendations and the findings that led up to them.

RANN Programs in the Social and Behavioral Sciences

It is harder to evaluate the RANN programs, especially in cases where the product is not in the form of publication, than to evaluate the basic science programs, and the Committee would not wish to claim that it has been able to carry out a detailed and exhaustive review. However, members of the Committee have examined a substantial number of RANN research products on topics on which they are knowledgeable, including some supplied by the Directorate itself as examples of the best work. The Committee has studied evaluative reports produced by the General Accounting Office and by RANN itself. It has examined the rosters of investigators funded by the RANN program and of referees who have participated in proposal review. It has received, by solicitation and spontaneously, a considerable number of comments from the applied research community about both RANN procedures and the quality of particular research products. Finally, it has interviewed RANN staff. These various sources provide a generally consistent picture. Some of our specific evaluative comments are included below in the descriptions of the several RANN programs, which are followed by a summary of our evaluation.

At present, RANN includes five major divisions: energy, environment, productivity, intergovernmental science and public technology, and exploratory research and problem assessment. For reasons stated previously, it is difficult to determine exactly what part of these divisions falls within the boundaries of the behavioral and social sciences. Among the 1975 programs in these divisions, the following were apparently either primarily social science activities or had a substantial social

science component: energy systems (energy division), managing the natural environment (environment division), disasters and natural hazards (environment division), public sector technology (productivity division), public policy and economic productivity (productivity division), public policy and human resources (productivity division), and technology assessment (exploratory research and problem assessment division). Some of these programs have recently been reorganized or changed in substance. The following discussion bears on programs described in the RANN 1976 budget submission to the Congress.

Energy Systems

All of the RANN energy programs contain as one component research on the social, economic, legal, and environmental aspects of the development of new energy sources. For example, research on geothermal energy is concerned with the ownership of energy rights, tax incentive policies, leasing arrangements, implications for land use patterns, and cost-benefit implications for the energy industry.

Among the energy programs, the energy systems program is especially involved in social science research, for it is concerned with evaluating, from technological, environmental, economic, social, regulatory, and legal standpoints, options for the future development of the nation's energy system. Research is being funded that deals with energy supplies and the sensitivity of supply to price, with cost-benefit analysis of potential energy savings through conservation measures, with the development of the data bases needed for effective national and regional energy planning, with the construction of modeling techniques for analyzing energy systems and their interactions with environmental, economic, and social variables, and with the application of energy models to policy problems.

Applied economics--but economics resting on technological foundations--is at the core of this program. The line of demarcation between the RANN energy research and research on the economics of energy funded by the economics program of the Division of Social Sciences is not at all clear to the Committee, and perhaps not to the respective program directors. It would appear, however, that the program in the Division of Social Sciences has been more successful than the RANN program in attracting

proposals from leading economists. The nature of the projects apparently encouraged by the RANN energy program may make it less attractive to those economists. In the fiscal 1976 RANN budget submission, four projects with substantial social science content are mentioned: initiation of a project to evaluate and incorporate the impact of geothermal development on local and regional planning; design of a reference energy system for the New York City area; construction of a model for natural gas policy studies; and research to inform legislation on energy matters introduced in the Texas legislature. Some of these projects appear to be consultations more than research projects, and others represent modest extensions of previous work.

Managing the Natural Environment

Social science research, particularly economic analysis, is especially relevant to systems studies of the effects of measures for the protection of the environment and cost-benefit studies of specific policy proposals in the environmental area. Examples of recent projects cited in the fiscal 1976 budget submission are cost-benefit studies of hail-suppression measures in the Great Plains and a computer-based data bank for processing siting permit applications received by the Corps of Engineers. Among projects planned for the coming year are the collection of data on land-use allocation and the economic aspects of environmental control for incorporation into models of particular environmental problems; analysis of local growth management strategies; and the development of environmentally sound and economically feasible methods for managing western range lands.

As with the energy program, there is an intermingling of economic and technological considerations that perhaps distinguishes this activity from most of the economics program of the Division of Social Sciences. There is some participation of distinguished agricultural economists and general economists in the program, but on the whole, the roster of researchers and the list of referees who evaluated the proposals were not impressive.

Disasters and Natural Hazards

Research on the social impact of natural hazards and disasters represents a relatively small part (less than $1 million out of $13 million) of the total program. The main objectives of the social impact component are to evaluate the costs and benefits of alternative policies for dealing with natural hazards and to increase the analytical base for dealing with low probability, very high-risk events. The program plans to approach the former objective with case study comparisons of different ways of providing disaster relief; comparative studies of building codes, land-use regulations, and zoning will be undertaken in order to identify damage-minimizing strategies and alternatives.

The study of decision making under conditions of high risk and great uncertainty is a topic of great importance from both a theoretical and applied standpoint. RANN has enlisted the efforts of some well-qualified investigators in this part of the program.

Public Sector Technology

Public sector technology means physical devices that can be used to improve public services as well as administrative, institutional, and legal practices in the public domain. Much of the work in this program involves operations research and management science, while other portions fall in the domain of law or public administration. Examples of recent projects include research in the use of videotape technology in courtroom trial situations and the development and application of systems for allocating police cars to patrol sectors. Much of the program, however, involves consultation or resource services rather than new research.

The range of social science skills involved in the different projects is very wide--from social psychology to management science in the two examples cited. This diversity serves as one example of the difficulties inherent in the present RANN program organization and the potential advantages of regrouping the research, as discussed below, to correspond more closely with the boundaries of the appropriate applied social sciences.

Public Policy and Economic Productivity

The four elements of the program are: measurement of the productivity of government services, effects of intergovernmental structure upon service productivity, effects of government regulations upon economic productivity, and problems of the post-industrial economy. The first three of these are standard topics in political science, public administration, and the economics of regulation.

Among recent activities reported are publication of a report on the legal, economic, and energy considerations of the use of underground space, a product liability study, the development of procedures whereby a local government can track the productivity of its programs over time, and an extensive series of studies of general revenue sharing. The output of many of these projects takes the form of informational reports for policy makers, which are precisely the kind of research product that investigators in these applied disciplines are accustomed to deliver.

On balance, the quality of the RANN products that were examined was not high. The reports on general revenue sharing, for examples, were based on computer runs showing the direct dollar effects of particular revenue allocation formulas, without any attention to the shifts in incidence resulting from economic and social reactions. Such indirect effects, which are standard topics of research in public finance, might substantially alter conclusions drawn by computing the direct allocations. It is difficult to distinguish the topics supported in this program from some of the topics supported by the economics, political science, and law and social sciences programs in the Social Science Division. The detailed nature of the work is different, however. Much of the RANN program is devoted to producing state-of-the-art papers or reviews of literature; those that were examined were of rather pedestrian quality.

Public Policy and Human Resources

This program, which lies almost completely in the social sciences, has two components: public policy and the disadvantaged and consumer market policy. The objective of the first is to analyze distributional and equity issues raised by the increasing proportion of aging and dependent persons in the U.S. population and by the special problems of minority groups. Research is planned on the adequacy of

public and private retirement systems, alternative policies for the support and the employment of the elderly, the changing role of women and the family, and the differential impacts of productivity change on minority groups. Little of this work has reached a stage of development at which it can be reviewed.

The consumer market policy research is aimed at developing information of use to policy makers concerned with the effective and efficient functioning of the consumer market for goods and services. Studies will be directed to such topics as the processes involved in setting product safety standards and the processes by which safety considerations are incorporated into design. Studies will also be undertaken of consumers' uses of information, product warranty practices, and improved measures of consumer welfare. These topics could be handled very well within the proposed realignment of RANN programs (see below) and the proposal review process would gain from closer contact with the relevant disciplines, sociology and economics (particularly labor economics and consumer economics).

Technology Assessment

According to RANN's fiscal 1976 budget document, "Technology assessment studies are designed to systematically explore and evaluate the social, economic, and environmental consequences of the introduction, extension or modification of major technologies." Among recent projects in this program are a technology assessment of the potential for mitigating water supply problems through snowfall enhancement in the Colorado River area, a study of the potential effects on society of some ten alternative automotive power systems, a study of the economic and social consequences associated with various alternative work weeks and work schedules, a feasibility study of using tactile sensory augmentation to aid dyslexic children in learning to read, and the development of a capability for analyzing the economic and environmental consequences of agricultural policies.

This program involves the same kinds of economics and other social sciences as several of the programs described above and raises the same questions about program organization. The heterogeneity of the program from a scientific standpoint is highlighted by the dyslexia study, which would presumably have to be evaluated by psychologists and educational specialists.

Intergovernmental Science and Research Utilization

This program combines two activities previously administered separately: intergovernmental science and experimental research and development incentives. The purposes of the program are to develop alternatives for state and local governments to use science and technology more effectively to facilitate the transfer and use of RANN research, to design incentives for overcoming barriers to research utilization, and to "provide user perspective" on RANN programs.

The Committee did not examine this program as a separate component, but instead as a part of RANN's overall utilization efforts. Our comments below on the Directorate's concept of research dissemination, utilization, and application and on its utilization activities are pertinent to this program.

Human Performance: A Neglected Research Area

Several instances have been mentioned of RANN research that is primarily psychological in content--for example, the use of videotape in courtroom procedures and the dyslexia study. However, there is now no program within RANN that is concerned with general problems of applied psychology, especially the part of applied psychology that studies human performance in such tasks as reading instruments or operating complex equipment in partly automated systems and investigates the effects of variables like age and aging on performance.

Human performance research made important contributions to the American military effort in the two world wars, as well as to our civilian society in incorporating human factors in the design of instruments and equipment. This kind of research has been mainly supported since World War II by agencies within the Department of Defense. During recent years these research programs have been curtailed and also directed much more narrowly than in the past to the specific short-run problems of the sponsoring mission agencies. Furthermore, research findings produced in specific military settings (e.g., recent work on the literacy of army recruits in different occupations) is not diffused as readily as it would be if it were produced by an organization concerned broadly with the same problems as they affect the entire society.

The Committee recommends that, as part of the proposed realignment of social and behavioral science programs within RANN, a broadened program be mounted in human performance research. As a component of this program, consideration should be given to the possibility of establishing one or more laboratories dedicated specifically to this topic in affiliation with universities. This pattern of organization has been followed in the United Kingdom, which has had considerable success in human performance research.

General Evaluation and Conclusion

The problems being addressed by RANN-sponsored research are generally important ones, and a substantial amount of useful work is being done. Nevertheless, it is the Committee's judgment, on the basis of the evidence it has examined, that the quality of the work is highly variable and on average relatively undistinguished, with only modest potential for useful application.

The programs are not enlisting as wide a range of participation from the social and behavioral science community as one could expect or wish. To some extent there may be a visibility problem, although at least economists and sociologists appear to be generally familiar with the existence of RANN and the character of its programs in their disciplines. In political science, the problem may be a little different: the RANN program addresses itself especially to topics of public administration and local government, which, from the 1950s until very recently, were attracting relatively few researchers. A factor that is probably more important than visibility is the perception by many social scientists of the RANN program as highly structured, supporting mainly one-shot, very short-range undertakings, and retaining in the agency the main responsibility for problem definition. Whether or not this perception is correct, if it is held by any considerable number of social scientists, as we think it is, it is damaging to the program.

It is also possible that there is a deficit of technology-oriented and technology-wise social scientists and some difficulties in mounting interdisciplinary efforts between natural scientists and engineers, and social scientists. It may be that many social scientists are uninterested in the kinds of applied problems represented by RANN's programs, but we are skeptical that this is a major

consideration. Programs in the basic research directorate having strong implications for application (e.g., the social indicators program and the economics program) have succeeded, generally, in attracting first-rate investigators. Moreover, as we point out repeatedly in this report, the topics covered by most of the RANN programs in the social sciences are topics that applied social and behavioral scientists are accustomed to addressing, and indeed, generally fall within established applied social sciences. For both of these reasons, we believe that there is an adequate supply of competent social and behavioral scientists with appropriate experience and interest to carry out major research programs on those topics.

The Committee concludes that the number of high-quality proposals submitted to RANN could be substantially increased and the proposal evaluation process could be significantly improved if the RANN organization and procedures for social and behavioral science research were realigned as is proposed in the following section: recasting the structure of the programs to correspond more closely with the applied social sciences, and placing more of the responsibility for program planning and problem definition upon members of those sciences working closely with representatives of potential user groups.

Routes to the Application of Knowledge

The present RANN program defines its objectives in terms of research results that might be applied to selected national issues. The assumptions on which the program now operates are that the products and users of research can be fully specified and the application of the research planned in detail before the research project begins. These assumptions fail to take account of the several ways in which knowledge in the social and behavioral sciences is produced, diffused, and then applied. There is no single path that leads from basic research to socially useful products, and the typical routes are perhaps more diverse in the social than in the physical and biological sciences. Among the alternatives are what might be called the "engineering model," the "medical model," and the "public diffusion model."

The physical sciences produce new knowledge that feeds through the engineering schools into industry, which applies it through additional technological research and development and new product design. NASA planning was

based largely on this engineering model, as are the present organization and policies of the RANN program.

The biological sciences produce new knowledge that feeds through the medical schools into medical practice, where it is applied by physicians in treating their patients. This medical model is appropriate for knowledge that is diffused by experts to trained practitioners. (Some important social science research has described how this process works in the development of new antibiotics and in the acceptance by farmers of hybrid corn.)

These routes from basic research to application have not been followed only in the biological and physical sciences. The development and application of new management science tools, for example, linear programing, essentially followed the engineering model: the tools were developed initially by statisticians, economists, industrial engineers, and organization theorists in the universities; introduced into business and engineering curricula; and brought into business firms by a new generation of employees and consultants.

There are, however, other paths to application. The typical product of basic research is publication in the open literature, where it is available to all, and the results of applied research may also follow this route. The general diffusion of knowledge through publication is especially important when the final application requires a large number of people, particularly if they are not professionals, to take action or to change their habits. For example, many Americans take daily vitamin supplements with their food. (We do not wish to argue that they take the right amounts!) Most of those people have certainly not read the original research reports upon which estimates of human vitamin requirements are based. Rather, those estimates have trickled down from their research sources through many steps of successive popularization, finally finding a place, for example, in standard cookbooks. General dissemination of information through many channels is what is called the "public diffusion model."

This kind of diffusion and popularization accounts for a very large part of the use and application of knowledge developed by the social and behavioral sciences. One reason for this is that much of such knowledge is important to us as individuals, as parents, and as citizens and participants in the political process. Research of behavioral scientists and others on the effects of one's life-style on health, for example, have been widely diffused to the public through textbooks, newspapers, and

magazines. Now, millions of Americans are better informed about the consequences of their sleeping, eating, and exercise patterns.

Probably even more Americans have gained through the public diffusion process information valuable to them as parents. Not so long ago, people viewed children as rather passive, inadequate adults, who require constant exhortation and derogation from parents and teachers and were expected to follow a course of lockstep development. Basic research on the physical, social, and intellectual development of infants and children has drastically changed this view. Cyclical fads and fashions in child rearing still exist, of course, but these should not obscure the progress that has taken place in public understanding of children's physical readiness, variability, need for intellectual stimulation, the importance of parents as models, and the value of motivation based upon feelings of competence.

Public diffusion also significantly influences the way knowledge from the behavioral and social sciences is most usually brought to bear on public policy issues--by developing new perspectives on public problems and alternatives. The work of anthropologists on African tribal boundaries and customs, for instance, has made a contribution to our national stance toward Africa by increasing public understanding of non-European traditions in general and of recent border disputes in Nigeria and Angola in particular.

These simple examples should warn us against exclusive attention to the engineering and medical models as the only models for applied science. The present RANN program of the Foundation places heavy emphasis upon identifying specific users for each project, specific research products that will be delivered to those users, and specific ways in which those research products will be applied. This may be a suitable formula for supporting certain kinds of research, but it is inappropriate for evaluating proposals in the social sciences that are important for public policy and whose normal, proper research product is knowledge published in the open literature and widely diffused.

Orientation of the Directorate

In the current conception of the RANN program, the main source of ideas for research are the program directors

and the communities of potential users of the research products. The organization is structured to identify applications in search of science, rather than science in search of applications. The planning process has a top-down character, with explicit provision for advice and feedback from the user communities, but relatively little from the scientific communities from which the investigators are drawn. Thus, NSF's statement in the fiscal 1976 budget submission to Congress that "RANN serves as a bridge between the Foundation's basic research programs and the development, demonstration, and operational programs of Federal mission agencies, State and local governments, and industry" is somewhat incongruent with the asymmetry of the organizational arrangements, which exaggerate the gap between the user community and the research community. For most areas of applied science, there are substantial numbers of research investigators who have a good understanding of the nature of the applied problems and close communications with the user communities. More effective use could be made of these scientists in program planning and evaluation and in project selection; we shall propose, below, a specific way in which this can be done.

The end-use-oriented character of the program is further, and properly, emphasized by the requirement that each project funded have a utilization plan. However, insufficient recognition appears to be given to differences in the meaning of utilization for different projects. In some cases, utilization means actually changing the programs of public or private organizations. In other cases it means securing the attention of administrators or legislators to published reports that are a research project's direct product. In still other cases--and this is especially but not exclusively true of evaluation research--utilization means broad dissemination of research findings to the people who will be concerned with the policy decisions to which the research relates. This would be true, for example, of the experimental research that has been carried out to evaluate the motivational effects of a negative income tax. The Directorate needs to give more attention to this wide range of meanings of utilization and to the problems involved in measuring utilization, particularly that which follows relatively indirect paths.

In the past year the Directorate has initiated a comprehensive evaluation procedure for program and project performance, the value of which cannot be judged at

this early date. However, for the reasons just stated, the Committee has some skepticism as to how effective it can be in the absence of more sophisticated techniques than RANN now employs for utilization research, an important component in the evaluation of an applied research program.

Management of the Directorate

The Directorate for Research Applications manages its program in a strikingly different way from the directorates responsible for the basic research programs. Although most of the funded projects result from unsolicited proposals, as in the basic research programs, a significant fraction are obtained through formal, competitive program solicitations and requests for proposals. Moreover, in most cases unsolicited proposals are really negotiated proposals. The Directorate itself defines explicitly a set of research objectives and actively seeks researchers through program announcements as well as through the solicitations and requests for proposals. One result of the difference in procedures is that the researchers are less often based in universities than is the case for basic research, and more often in nonprofit or profit-making corporations. Another result, which is basically undesirable, is that projects, as finally defined for funding, often incorporate more of the conceptions of the Foundation's staff than of the investigators who are expected to carry them out.

Stability of Programs

In almost all of the Foundation's research programs, basic as well as applied, research grants are awarded for only short periods, seldom more than one or two years. In the basic research programs, however, the presumption prevails more frequently than in RANN that if the quality of the research done on a project is very good and promises to continue to be good, the support will probably be renewed. Under these arrangements, excellent work can continue to be supported as long as it remains excellent, but all projects are reviewed periodically to determine whether they deserve further funding.

The opposite concept appears to prevail in the Directorate for Research Applications, at least in the way

in which it approaches its task: it is assumed that the research project is to deliver a specific product at its termination date, at which time the work will come to an end. If this concept were interpreted literally, the time horizons of investigators would be very short indeed. Having accepted a research grant, they would have to form a research group, carry out the project, and liquidate the group, all in the space of 18-24 months. First-rate investigators are not likely to be tempted to engage in research under such conditions, and if they are tempted, will not do their best work.

Nor is it reasonable to assume that solutions will be found for important applied problems in the space of a year or two. A much more reasonable assumption would be that within a long-term research program, intermediate goals can be set so that tangible products can be delivered and evaluated periodically. High current productivity of a research program should be a reason for continuing it, not for terminating it when it reaches an intermediate goal.

Fortunately, the actual practice in RANN has been rather more flexible than the concept described above. In numerous instances it has been recognized that applied research does not necessarily, or even usually, lead to a single product that solves a problem, but more commonly produces a series of outputs, the quality of which is likely to benefit from continuity of support in the same way as does the quality of the outputs of basic research. We note that in a substantial number of cases the Directorate has renewed project support one or more times, but in our judgment there is less continuity, on the whole, than is desirable. It is easier to ensure continuity when projects originate in unsolicited proposals than when they are funded after formal solicitations, for in the former case the projects are more likely than in the latter to fit into the long-range research plans and interests of the investigators. This is another reason for favoring unsolicited proposals as the principal means for generating projects.

The present RANN conception of attacking specific research goals, reaching them, and then setting them aside for new goals has led in other ways to continuing fluctuation and instability in its programs. The Directorate has been oversensitive to the public problems of the moment and has been unwilling or unable to provide the lead time to anticipate problems that are near in prospect, but not yet urgent. Basic human and social problems do

not appear one year and disappear the next, never to be seen again. Most of the current problems of our cities--including transportation, finance, and housing--can be found described in familiar terms in the municipal government literature of the 1930s. Severe water and air pollution problems were with us 40 years ago and will be with us, albeit in different forms, 40 years from now. The prospect of an energy crisis was apparent to anyone who cared to look at the situation a couple of decades ago.

This does not mean that problems cannot be solved or are never solved. It means that there are large and complex problems that have to be dealt with by attacking a succession of component subproblems. It also means, as has already been emphasized, that a considerable part of applied research consists of continuing evaluation, along important social and human dimensions, of the consequences of the many changes that are taking place in each problem area. Hence, it is reasonable to build research capabilities for applying scientific knowledge to these multifaceted problems and to use the research capabilities to tackle a whole sequence of components of these problems over a period of years.

RANN should support a mix of projects, including research involving applications of known technology, research closely related to possible applications, and research on the process of utilizing knowledge and how such utilization can be facilitated. In the social and behavioral sciences, it is likely that the mix will be much more heavily weighted with the second and third, since there are relatively few social problems for which known facts lead to solutions that can be easily applied. A more reasonable process would begin with a period of research phasing into small-scale experiments to develop better evidence and better ways of implementing the social changes that most applications of research require. Such experiments would be followed by larger-scale application. Throughout, a continuing process of feedback, adaptation, improvement, and learning would be maintained. The past decade's history of programs and experiments that did not lead to the expected results attests to the need for a revised approach to research application in RANN and elsewhere.

A Management Alternative

Several of NSF's basic research programs provide a useful model of the way in which problems for applied

research could fruitfully be attacked. We observed earlier that for many social science topics, the dichotomy between basic and applied research cannot be taken literally. The social indicators program, for example, can be viewed as both an effort to define and measure basic social magnitudes--hence a basic research effort--and an effort to provide measuring instruments for examining the quality of life and its relation to government policies and programs. Research targets are defined both in terms of needs for application and in terms of the level of knowledge and techniques available for reaching them. Initiatives for developing specific research have come largely from the social science disciplines, and some of the long-term planning functions that could be performed by an advisory panel are performed by a committee of the Social Science Research Council. There is a reasonable expectation of continuity of support for the projects.

Even viewed as a research applications program rather than one aimed at advancing basic knowledge, the managerial style of the social indicators program seems to us more effective and successful than the style employed by the Directorate for Research Applications. We think the same claim could be made for some of the projects within the economics program: for example, the construction of macroeconomic models of national economies for purposes of prediction and policy guidance.

Perhaps the deficiencies of the RANN managerial style are peculiar to the social sciences and this style is very well suited to research applications in the physical, engineering, and life sciences. Our observations and recommendations here are limited to the social and behavioral sciences.

Use of the Applied Science Community

Each RANN program director faces, or should face, in two ways--toward a user community that can help determine the relative priorities among different problem areas and toward the applied science communities that must provide the research skills for the program. Currently, the user community is not at all well defined for most of the RANN programs as presently structured, nor is there a clear-cut scientific reference group for each program. As a result, the program directors have very large margins of discretion in formulating programs and choosing projects and few places to turn for help in exercising that discretion

wisely. The potential research community is sometimes tapped to assist in formulating program solicitation documents, which undoubtedly improves those documents, but has the unfortunate result of excluding from the competition for awards the very persons whose competence caused them to be asked to help define the research problem.

These weaknesses in the organization and operation of the RANN program could be remedied if the Directorate took account of the fact, now formally ignored, that there already exist interdisciplinary communities of applied social science investigators who are working on topics closely related to most of the RANN programs. Those investigators are interested in tackling applied problems, are in constant and close communication with potential users, and are collectively in a better position than a handful of RANN program managers to identify and define the promising applied research problems. For example, the long-established community of researchers in public finance is well motivated and qualified to formulate its own research studies and to interact with the user community (as it does regularly) on such topics as revenue sharing, without close research direction from the RANN professional staff. Or, as another example, the problems of governmental regulation of business have long been dealt with by investigators in the closely related fields of public law and industrial organization. The difficulties in the present style of management largely derive from the faulty assumption that the program must be planned from above, that projects must first be defined almost independently by the user community and the RANN staff, and that investigators must then be induced to carry them out within a prestructured framework.

It is important for NSF to differentiate its role in applied research from the role of mission agencies. It can do so--taking advantage of its primary position in support of basic research while carrying out its task to undertake research relevant to national needs--by concentrating on problems whose solutions require more basic understanding of processes and techniques of application and on problems that involve the interests of more than one agency. Such problems fall in topic clusters that are the concern of existing communities of applied social and behavioral scientists. Almost all of the current RANN projects in these sciences relate to established interdisciplinary applied social science areas, such as: public finance, organizational administration, operations research and management science, communication, public

choice, urban affairs, human performance, land use and resource management, government regulation of industry, public law, and program evaluation and measurement.

With an appropriate definition of the RANN mission, much of its applied social and behavioral science research could readily be reorganized into a set of programs, each of which would cover one or more of the topics listed above. The program manager of each such program would be advised by a program review panel operating in exactly the same way as has been recommended for the basic research programs. The membership of the panel would be drawn from both the user community and the relevant applied sciences. The program director and panel would be responsible for long-term planning, as we have recommended for the basic research programs, but would depend primarily on unsolicited proposals--and this would now mean genuinely unsolicited proposals--as the principal source of projects to be funded.

Such an arrangement would be substantially more effective than the present one for tapping the collective wisdom of user and scientific communities in order to identify the most important undertakings and would produce higher levels of motivation among researchers and dedication to the goals of helping to solve social problems. The Committee believes that it would bring to attention important research problems that are missed by a top-down planning process and that it would lead to greater consistency in pursuing applied research goals over longer time spans. It would certainly reduce the tendency to jump into and out of research areas on the basis of momentary shifts in public attention.

With such an arrangement, RANN could truly serve as a "bridge between the Foundation's basic research programs and the development, demonstration, and operational programs of Federal mission agencies, State and local government, and industry." Across that bridge, ideas would flow in both directions--something that is very difficult with the present asymmetric arrangement. It would be easier to design in-between projects, like the social indicators project already mentioned, tool-development projects, and anticipatory research directed at emerging problems.

Staffing

One hears frequent comments from the present RANN staff that the organization is understaffed. This is almost surely the case, given the enormous responsibilities for initiation and program definition, as well as proposal review, that are placed on every program director. A large part of the staffing problem would be solved by adopting, as recommended, the management style of the basic research directorates, so as to reduce staff responsibilities to reasonable proportions.

One other staffing problem should be mentioned. Those parts of the Directorate for Research Applications that manage social and behavioral science research projects are staffed mostly by administrators who have training in those sciences. Those parts of the Directorate that manage mixed projects--technical projects with an economic or social component--are staffed almost exclusively by physical scientists. To deal with topics of these kinds realistically almost always calls for serious and continuous concern with the costs and benefits of proposed technological solutions and the behavioral changes they will produce or that will be required in order to implement them; this is a major concern of behavioral and social scientists. Finally, there are almost no social and behavioral scientists in administrative positions above the divisional level. Under these circumstances, there are severe organizational problems in bringing expert judgment to bear on some of the organization's crucial decisions having a large social or behavioral component.

A New Division

Reorganization of the programs as recommended would lead quite logically to the creation of an applied social and behavioral science division within RANN, which would provide some additional social science leadership at a high level in the organization. The reorganization would not by itself solve the problem of providing better social and behavioral science participation in programs with a combined technical-social content, but the new division could play a useful advisory and liaison role in securing appropriate social science inputs to technological projects.

OTHER SOCIAL SCIENCE PROGRAMS

In addition to social and behavioral science research funded by the Directorate for Biological, Behavioral, and Social Sciences and the Directorate for Research Applications, there are significant activities in these fields elsewhere in the Foundation--in the former Office of National R&D Assessment, the Directorate of Science Education, and the Division of Science Resources Studies. Limits of time and knowledge have prevented the Committee from giving more than a cursory glance at these programs, which largely fall outside the NSF's central mission to support basic and applied research.

Office of National R&D Assessment

The mission of this office has been to conduct research on technological innovation that will be useful to help guide government science and technology policy.

The office carries out research both intramurally and extramurally, with extramural research accounting for perhaps three-quarters of its total activity. Both solicited and unsolicited proposals are received, the latter being negotiated to adapt them to the office's program. The extramural program has attracted proposals from a number of first-rate economists and other social scientists, and the general quality of the proposals that have been funded is very good. The intramural research appears to consist primarily of the development of policy-oriented position papers for internal government use.

The Office of National R&D Assessment has been combined with the Science and Technology Policy Office and the Office of Energy R&D Policy in the new Division of Policy Research and Analysis. Six working groups will

operate within that Division, dealing with: the effects of public policy on science and technology, processes of innovation and their management, socioeconomic effects of science and technology, emerging science and technology, international science and technology policy, and policy aspects of energy, resources, and environment. The first three correspond roughly to the activities of the former Office of National R&D Assessment. A somewhat broader focus and an increased use of ad hoc task forces is planned.

Science Education

The Comittee has not made an examination of the Foundation's programs in behavioral and social science education. Apart from questions of their scientific content, which we might feel qualified to evaluate, these programs raise a number of issues of pedagogy and of general governmental policy that this Committee is not especially constituted to deal with. Moreover, some aspects of the program have received recent careful study by the Comptroller General's Office (*Administration of the Science Education Project "Man: a Course of Study" [MACOS]*), and by the ad hoc Science Curriculum Implementation Review Group appointed by the chairman of the House Committee on Science and Technology.

Science Resources

One NSF unit is responsible for gathering data and producing reports on scientific and technical personnel with the aim of informing policy decisions. As with the education programs, the Committee does not believe that it is appropriately constituted to evaluate this program, which lies outside the main research support mission of the Foundation.

IMPACT OF RESEARCH POLICIES ON UNIVERSITIES

The issue to be discussed in this final section is not peculiar to the behavioral and social sciences, but involves all university research supported by the Foundation. The issue is of fundamental importance to the continuing success of all of the Foundation's programs.

In general, the Foundation supports only a fraction, and often a small fraction, of the research activities to which it makes grants. The costs to a university of carrying on research include: (1) supplies and equipment; (2) faculty salaries during the summer months; (3) faculty salaries during the academic year; (4) the salaries of technicians, research assistants, and other support personnel; and (5) overhead costs. Because of the usual problems of separating joint costs, it is not easy to determine total project costs exactly. But it may be said in general that project budgets tend to cover the largest part of the costs in the first, second, fourth, and fifth categories, but very little of the cost of the third.

As research funds have continued to shrink over the past seven or eight years at the same time that price levels have risen substantially, the average size of project budgets has been approximately stationary. Since the number of projects supported has increased, this has created the illusion that the Foundation is continuing to increase the range of work it is supporting. What it means in fact is that each Foundation grant to a university has been exacting a larger and larger subsidy from the university's own resources.

It is not generally recognized what a small part of the cost of the scientists engaged in research on NSF projects is supported by the NSF grants. Data contained in the budget submission of the Division of Social Sciences show that in fiscal 1975 the division made 618

grants, totaling about $26.3 million, or about $42,500 per grant. Of this total expenditure, only 27 percent, a little more than one-quarter, represented the cost of the scientists supported by the grants. A total of only 335 man-years of scientific effort was supported, about one-half person per year per grant. We do not have corresponding data for universities, but from our own experience and knowledge of project budgets, we judge that universities support at least as many professional staff as does the NSF.

As university enrollments stabilize and then decline, as they are almost certain to do in the next decade, we are going to find ourselves on a collision course between the shrinking revenues of universities and their responsibilities for discovering new knowledge and applying it to society's problems. We cannot avoid the conclusion that, while certain improvements that we have recommended in this report can be made in the social and behavioral science programs of NSF within the present budget constraints, unless there are substantial increases in funding, much needed research will simply not be done.

APPENDIX A

PUBLICATION OF BASIC RESEARCH SUPPORTED BY THE NATIONAL SCIENCE FOUNDATION

One indirect index of the quality of basic research supported by the National Science Foundation is publication in scientific journals. Articles submitted to highly regarded journals receive competitive editorial review by other scientists. If NSF-supported research is of high quality, the percentage of published articles reporting research funded by NSF should approximate the percentage of researchers supported by the Foundation. This appendix compares the fraction of articles in various disciplines that acknowledged NSF financial support with the fraction of researchers in those same disciplines whom NSF supports.

The data must be interpreted with care and with proper qualifications. Publication of research cannot be taken as a direct measure of the quality of the Foundation's decisions. First, researchers who receive grants would be expected to be more productive than those who do not. Moreover, the research supported by NSF might be of higher quality than that supported by many other agencies because excellent researchers are attracted by the flexibility of the Foundation's program. These and other difficulties in interpretation limit the conclusions one might draw from the data. They cannot show that those not supported might not have published more had their research been funded by NSF. Whatever the direction of causation, however, if NSF supports research of high quality, then that research should show up prominently in the scientific journals.

Three or four top-ranked journals in each of several disciplines were examined. In three issues of each journal in fiscal 1971 and 1975, the Committee counted the number of articles acknowledging, and the number not acknowledging, partial or total support by the Foundation.

Next, estimates were made of NSF research funding and total federal funding in the selected disciplines two fiscal years before the publication dates of the journals examined, that is, in 1969 and 1973. (*Federal Funds for Research, Development, and other Scientific Activities*, various years). The two-year lag was taken as the minimum time between the funding of a project and its published output. Another estimate was made of funding in 1973 by comparing the number of awards made by NSF for basic research with the number of doctoral scientists at universities and colleges who reported basic research as a primary or secondary work activity (*Reviews of Data on Science Resources*, 1975).

Table A1 shows comparisons between (a) the percentage of articles acknowledging NSF support and (b) the percentage of federally funded research support provided by NSF. One estimate of NSF research support was based upon the proportion of funds allocated to universities and colleges and another upon the proportion of total basic research funds provided by NSF. The data show a high degree of correspondence between NSF financial support and publication in the most highly regarded journals.

Table A2 reports comparisons between (a) the percentage of all published articles examined for fiscal 1973 attributing support to NSF and (b) the estimated percentage of basic researchers who were supported by NSF in fiscal 1973. Data were not available for 1969 or for disciplines other than those listed. To estimate the number of researchers supported by NSF during 1973, the total number of awards granted in fiscal 1972 and fiscal 1973 was counted. Table A2 also shows that research support by NSF is reflected in journal publication.

A final comparison was made between financial support by NSF and publication in more poorly regarded journals (ranked 15th to 25th). In all fields but economics, relatively few articles in these journals reported NSF support--from zero to eight percent. In economics, NSF-supported articles appeared about as frequently as in highly rated journals, but in economics there is very great pressure on the few excellent journals, and the overflow goes to other journals. On the whole, the comparison indicates that NSF-supported research is published in relatively high-quality journals, as would be expected if the research supported is of high quality.

TABLE A1 Evidence in the Literature of NSF Support

Field of Science	NSF Percentage of Federally Funded Research: At Universities, Fiscal 1969	NSF Percentage of Federally Funded Research: At Universities, Fiscal 1973	NSF Percentage of Federally Funded Research: Total Basic Research, Fiscal 1969	NSF Percentage of Federally Funded Research: Total Basic Research, Fiscal 1973	Articles Reporting Financial Support, Percentage Attributing That Support to NSF: 1971	Articles Reporting Financial Support, Percentage Attributing That Support to NSF: 1975
Psychology[a]	n.a.	22.5	11.8	19.6	26.3	23.8
Economics[b]	n.a.	20.7	17.5	21.3	38.6	52.1
Anthropology[c]	n.a.	71.9	37.4	36.4	37.9	37.1
Sociology[d]	n.a.	9.9	10.8	20.9	30.8	33.3
Political Science[e]	n.a.	89.7	40.8	53.4	40.7	18.8

[a]For each fiscal year, three issues (one each in fall, winter, and spring) of four top-ranked psychology journals were examined: *Journal of Comparative and Physiological Psychology* (1st), *Journal of Personality and Social Psychology* (2nd), *Psychological Review* (3rd), and *Journal of Experimental Psychology* (4th). Ranks were derived from ratings by department chairmen (Mace & Warner, 1973). If articles published by foreign researchers are subtracted, the percentage of supported research articles attributable to NSF increases.

[b]For each fiscal year, three issues of four top-ranked economics journals were examined: *American Economic Review* (1st), *Econometrica* (2nd), *Journal of Political Economy* (3rd), and *Review of Economics and Statistics* (5th). Ranks were derived from ratings by 111 academic economists (Hawkins, Ritter, & Walter, 1973). A substantial number of articles in these journals are theoretical and list no financial support.

[c]For fiscal 1971, three issues of two journals were examined: *American Anthropologist* and *American Antiquity*; for fiscal 1975, three issues of a new journal, *American Ethnologist*, were included. These journals were examined at the suggestion of the American Anthropological Association's executive officer. A substantial number of articles in the *American Anthropologist* are theoretical and list no financial support.

[d]For each fiscal year, three issues of four highly regarded journals were examined: *American Sociological Review*, *American Journal of Sociology*, *Sociometry*, and *Demography*, as suggested by the American Sociological Association's executive officer, but see also note *e*.

[e]For each fiscal year, three issues of three top-ranked political science journals were examined: *World Politics* (1st), *American Political Science Review* (5th), and *Journal of Politics* (6th). Ranks were derived from ratings made by students and department chairmen (Giles & Wright, 1975). Two sociology journals and one law journal in which political scientists publish (*American Sociological Review*, *American Journal of International Law*, and *American Journal of Sociology*) were ranked in 2nd, 3rd, and 4th place.

TABLE A2 Percentage of Scientific Articles Attributing Support to the National Science Foundation and Percentage of Basic Researchers Receiving Support by the National Science Foundation

Field of Science	Percentage of Total Scientific Articles Attributing Support to NSF (fiscal 1975)	Estimated Percentage of Basic Researchers Supported by NSF[a] (fiscal 1973)
Psychology	18.0	8.0
Economics	21.0	10.6
Sociology and Anthropology	18.5	16.4

[a]Summary tables of proposals and awards were provided by NSF. Awards in psychology were estimated to equal the number of awards provided by the social psychology and psychobiology programs.

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APPENDIX B

SUPPORT FOR YOUNG RESEARCHERS IN THE BEHAVIORAL AND SOCIAL SCIENCES

It is difficult to find accurate information on the age of researchers supported by the National Science Foundation and other agencies since these data are not routinely coded, stored, or summarized. Nevertheless, the Committee undertook a review of the available information on research support for young behavioral and social scientists after receiving several inquiries from colleagues regarding such support. From three sources--a survey of university department chairmen, reports of the National Register of Scientific and Technical Personnel, and a sample of principal investigators in the NSF--a generally consistent pattern emerged of declining support for young researchers, particularly in psychology and sociology.

A sample survey of department chairmen in 160 doctorate-granting institutions provides one indication of the research support status of young and senior scientists. The survey, conducted in spring 1974, covered 1,366 departments in 15 disciplines and fields, among them economics, psychology, and sociology. Faculty who received their Ph.Ds on or after July 1967 were classified as "young faculty," others as senior faculty. The two groups were compared with respect to the percentage of those who carry out research, whose research was supported, and who perceived the need for greater research funding. The findings on both groups were compared, on the basis of a matched group of 608 departments surveyed in both years, to the results of a survey conducted in 1968.

Several results have implications for the social and behavioral sciences:

1. A greater proportion of young than senior faculty was engaged in research; this difference was about the same in 1968 as in 1974 (Table B1).

TABLE B1 Proportion of Investigators in Doctorate-Level Departments Who Spend 20 Percent or More Time in Research

	Years Since Doctorate			
	7 Years or Less		More than 7 Years	
Fields	1968	1974	1968	1974
All Fields	90.9	90.8	84.0	85.0
Economics	89.0	82.3	80.3	77.2
Psychology	84.6	88.2	78.1	77.6
Sociology	81.7	91.5	77.1	88.4

TABLE B2 Proportion of Young and Senior Investigators in Doctorate-Level Departments, 1974

	Years Since Doctorate	
Fields	7 Years or Less	More than 7 Years
All Fields	30.1	68.2
Economics	37.8	58.7
Psychology	40.2	59.0
Sociology	42.5	52.1

2. Among faculty spending 20 percent or more of their time on research, about one-third were young investigators, and the proportion was even higher in the three social sciences (Table B1).

3. Young investigators in all disciplines and fields accounted for one-fourth of those doing research connected with federal grants and contracts--a proportion below the percentage of young investigators in the total faculty. The pattern was the same in the three social sciences, but the discrepancy was greatest in psychology: while 40 percent of all psychology researchers were young (Table B2), only 29 percent of those supported were young (Table B3).

4. Since 1968, the proportion of faculty doing research with federal support has declined except in chemistry, engineering, and economics; the greatest decline was in psychology (Table B4).

5. Most department heads (72%) thought the distribution of funds between young and senior faculty to be appropriate, although the percentages reporting that belief were slightly lower in the social sciences. Of those not finding the distribution appropriate, a majority favored special funding programs for young faculty investigators; the percentage was much higher than average in psychology (78%).

6. A majority of department heads said that faculty investigators were generally able to secure support. But in psychology and sociology, the percentage saying that young investigators had difficulty obtaining support was higher than average, and the percentage reporting difficulty for senior investigators was lower. The pattern of greater difficulty for younger than for senior investigators was found in all departments including those rated distinguished or strong.

The results of this survey must be interpreted with caution. Only three behavioral and social science disciplines were surveyed, and the data are based upon the self-reports of department chairmen rather than actual funding figures. It is possible that the alleged problems that young psychology researchers and some young sociology researchers have in obtaining funds reflects a special need for equipment or other support facilities not recognized by funding agencies. They may also reflect competition among a large number of recent entrants into research. However, special tabulations from the National Register of Scientific and Technical Personnel for 1964-1970 shows a relative lack of support for young

TABLE B3 Young and Senior Researcher as a Proportion of All Researchers Directly Connected with Federal Project Grants and Contracts, 1974

	Years Since Doctorate	
Fields	7 Years or Less	More than 7 Years
All Fields	24.9	74.1
Economics	34.8	63.6
Psychology	28.6	71.0
Sociology	38.5	58.4

TABLE B4 Young and Senior Investigators Directly Connected with Federal Project Grants and Contracts as a Proportion of All Faculty in Doctorate-Level Departments

	Years Since Doctorate			
	7 Years or Less		More than 7 Years	
Fields	1968	1974	1968	1974
All Fields	58.5	47.7	71.0	61.9
Economics	24.5	25.3	22.7	32.0
Psychology	57.1	30.6	65.3	53.9
Sociology	30.0	26.6	44.0	31.0

TABLE B5 Distribution of NSF Principal Investigators by Years Since Ph.D

	Year Grant Funded and Field of Science					
	Fiscal 1965			Fiscal 1974		
Years Since Ph.D.	Anthro-pology	Sociology, Social Psychology, Political Science	Total	Anthro-pology	Sociology, Social Psychology, Political Science[a]	Total
1-8	39.0%	38.9%	39.0%	25.6%	23.2	23.6%
9-16	26.8	31.0	28.8	31.6	30.9	30.7
17-24	11.4	9.7	10.6	18.4	23.2	21.1
25 or more	10.6	4.4	7.6	14.5	10.7	12.1
Unknown	12.2	15.9	14.0	9.9	11.9	12.4
Total[b]	100.0	100.0	100.0	100.0	100.0	100.0
(Total Number)	(123)	(113)	(236)	(154)	(168)	(322)

SOURCE: Carlos E. Kruytbosch, National Science Foundation, 1976.

NOTE: Percents may not equal 100 due to rounding.

[a]Comparisons among these fields are available for 1974, by which time there were separate programs for each. These figures show that the proportion of awardees who had their Ph.D degrees 8 years or less was 33.3 in political science, 23.9 in sociology, and 13.2 in social psychology.

researchers in the behavioral and social sciences in 1964, before the rapid increase in Ph.Ds.

The data from the National Register also show that federal support for young investigators engaged in basic research decreased from 64 percent in 1964 to 50 percent in 1970, while the decline in the proportion of senior investigators receiving such support was smaller. The decline in support for young researchers was particularly great in mathematics, social sciences, and psychology.

The available evidence on National Science Foundation programs gives a similar picture of declining support for young behavioral and social scientists. A comparison of the 1965 and 1974 NSF principal investigators in four social sciences shows that 11 years ago 39 percent of those receiving the basic research grants had held their Ph.D degrees for eight years or less, whereas in 1974 this proportion had declined to less than 24 percent (see Table B5).

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APPENDIX C

FACILITIES AND EQUIPMENT SUPPORT IN THE BEHAVIORAL AND SOCIAL SCIENCES

In a recent survey of department heads at universities, a relatively high proportion in psychology and sociology departments reported that investigators were having increasing difficulties in obtaining support for their research. The problem appears greatest for young researchers and for both young and senior researchers in departments rated strong or distinguished according to a study by Roose and Anderson (1970). Of those reporting difficulties, the department heads in psychology especially recommended special programs for young researchers and for equipment (Table C1). These opinions appear to reflect a relative lack of funding for equipment and facilities for research in which the need for these tools has increased rapidly in the past few years.

In the last ten years, major topics of research in psychology and social sciences have been revolutionized by 1) the use of computers for both simulation and operating laboratory experiments; 2) the development of sophisticated statistical techniques requiring computers; 3) the use of laboratory equipment to record physiological responses (e.g., the psychoacoustic equipment discussed elsewhere in this report); 4) the use of equipment for measuring overt behavior (e.g., speech pattern analysis equipment and eye movement cameras); and 5) the use of equipment permitting unobtrusive, but more objective, study of group interaction, attitudes, and thinking (e.g., videotape equipment). During the same period, however, the proportion of both total and federal funds for facilities and equipment allocated to the behavioral and social sciences has decreased.

Even a decade ago, expenditures for facilities and equipment in the behavioral and social sciences were relatively low. As illustrated in Table C2, expenditures in universities and colleges for these sciences were just

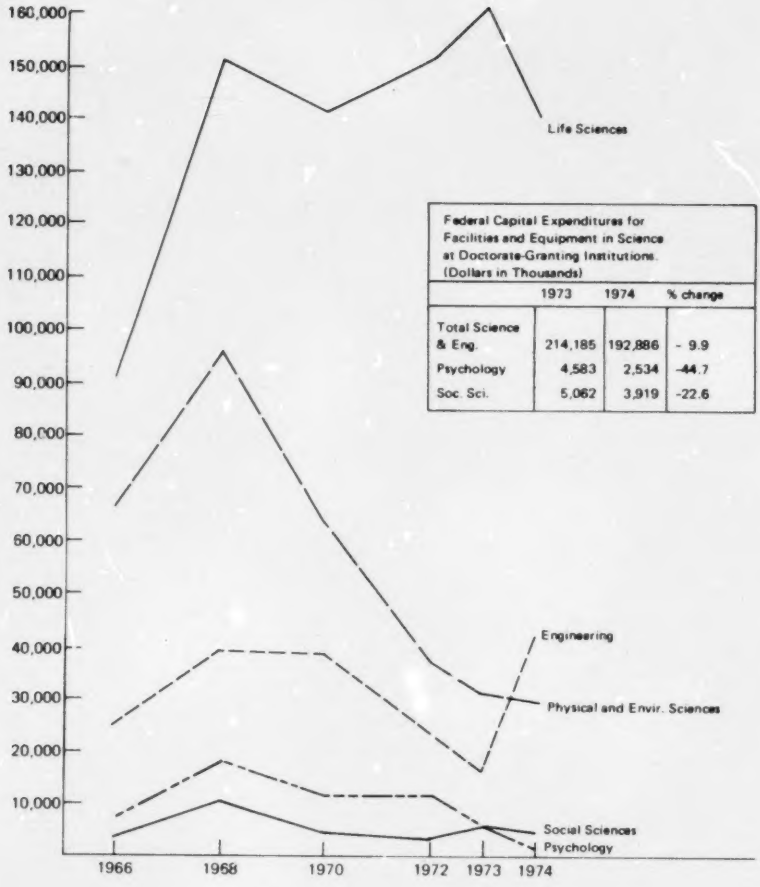
TABLE C1 Opinions of Department Heads Concerning Special Programs for Faculty Researchers, 1974[a]

Fields of Science	Percent Favoring Special Programs			Percent Opposed to Special Programs
	For Young Faculty Only	For Senior Faculty Only	For Special Equipment	
All Fields	52.2	3.4	70.7	42.4
Economics	41.7	8.3	16.7	45.8
Psychology	78.1	3.1	80.8	18.8
Sociology	53.8	3.8	40.0	42.3

[a]The question regarding special programs was asked to only those department heads who thought that the present division of funds between young and senior investigators was inappropriate (n = 156, or 27% of those surveyed).

TABLE C2 Capital Expenditures for Scientific and Engineering Facilities and Equipment, Fiscal 1964 and Fiscal 1974

Source and Field	1964	1974
All capital expenditures		
All sciences and engineering	$529,492,000	$837,334,000
Psychology	21,720,000	15,606,000
Social Sciences	32,588,000	59,478,000
Federally financed capital expenditures		
All sciences and engineering	134,439,000	227,694,000
Psychology	2,121,000	2,596,000
Social Sciences	2,020,000	4,614,000
Nonfederally financed capital expenditures		
All sciences and engineering	395,053,000	609,640,000
Psychology	19,599,000	13,010,000
Social Sciences	30,568,000	54,864,000



Federal Capital Expenditures for Facilities and Equipment in Science at Doctorate-Granting Institutions. (Dollars in Thousands)

	1973	1974	% change
Total Science & Eng.	214,185	192,886	- 9.9
Psychology	4,583	2,534	-44.7
Soc. Sci.	5,062	3,919	-22.6

FIGURE C1 Federal Capital Expenditures for Facilities and Equipment in Science

TABLE C3 R&D Expenditures by Federally Funded National Laboratories, 1974

Field of Science	Expenditure
Total, All Sciences	$865,098,000
Psychology[a]	850,000
Social Sciences[b]	330,000

[a]The two laboratories supporting some psychology are Lincoln Laboratory ($787,000 or 1% of their expenditures) and Oak Ridge Associated Universities ($63,000 or 17% of their expenditures).

[b]The two laboratories supporting some social science are Brookhaven National Laboratory ($14,000 or .02% of their expenditures) and The Center for Naval Analysis ($316,000 or 3.3% of their expenditures).

10.3 percent of total expenditures. By 1974, the proportion had declined further, to nine percent of the total. This decrease may be attributed mainly to the dramatic drop in facility support from non-federal sources in psychology, during a period in which federal support did not increase (and decreased if measured in constant dollars). Moreover, federal support continues to decrease. From 1973 to 1974, federally financed capital expenditures for facilities and equipment in science decreased ten percent; in psychology, they decreased nearly 45 percent; and in the social sciences, almost 23 percent (See Figure C1).

Another potential source of equipment and facilities is national research laboratories funded by the federal government, but national laboratories operated primarily for the support of behavioral and social science research do not exist. Further, only four of the 22 national laboratories allocate any funds to the behavioral and social sciences; the total allocated is just one-tenth of one percent of their expenditures (See Table C3).

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